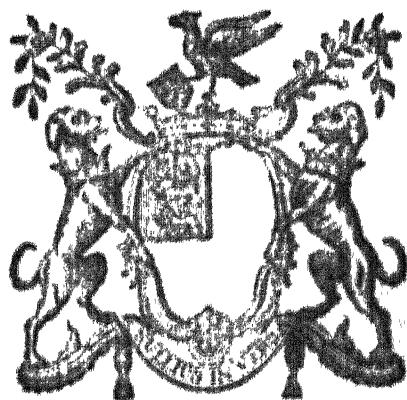# Agricultural Research Institute

PUSA

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

O F

LONDON.

VOL. LXVII. For the Year 1777.

P A R T I.

It is likewife neceffary on this occafion to remark, that it is an efta-blifhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any fubject, either of Nature or Art, that comes before them. And therefore the thanks, which are fre-quently propofed from the chair, to be given to the authors of fuch pa-pers, as are read at their accuftomed meetings, or to the perfons through whofe hands they receive them, are to be confidered in no other light than as a matter of civility, in return for the refpect fhewn to the Society by thofe communications. The like alfo is to be faid with regard to the feveral projects, inventions, and curiofities of various kinds, which are often exhibited to the Society; the authors whereof, or thofe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheft applaufe and approbation. And therefore it is hoped, that no regard will here-after be paid to fuch reports, and public notices; which in fome inftances have been too lightly credited, to the difhonour of the Society.

rative

CONTENTS.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a Woman in the Shire of* Rofs *living without Food or Drink.* By Dr. Mackenzie, *Phyfician at* New Tarbat. *Communicated by the Right Honourable* James Stewart Mackenzie, Lord Privy Seal of *Scotland.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Hill-Street,
June 5, 1776.

S I R,

Read Nov. 7, 1776. I HERE inclofe the cafe of the fafting woman in Rofs-fhire, authenticated by Mr. MAC LEOD the fheriff, and feveral juftices of the peace of that county; that, in cafe you fhall think it worthy to have a place in the Philofophical Tranfactions, it may be inferted therein. I am, with great regard, SIR,

Your moft obedient humble fervant,

J. S. MACKENZIE.

JANET MAC LEOD, unmarried, aged thirty-three years and fome months, daughter of DONALD MAC LEOD, tenant in Croick, in the parifh of Kincardine, and fhire of Rofs; in the fifteenth year of her age had a pretty fharp epileptic fit: fhe had till then been in perfect health, and continued fo till about four years thereafter, when fhe had a fecond fit, which lafted a whole day and night; and a few days afterwards, fhe was feized with a fever of feveral weeks continuance, from which fhe had a flow and very tedious recovery of feveral months.

During this period fhe loft the natural power of her eye-lids, was under the neceffity of keeping them open with the fingers of one hand, when fhe had any thing to do with the other, went out, or wanted to look about her; in every other refpect fhe was in health and tolerable fpirits, only here it may be fit to remark, that fhe never had the leaft appearance of the *menfes*, but periodically fpit up blood in pretty large quantities, and at the fame time it flowed from the nofe. This vicarious difcharge, according to her mother's report, happened regularly every month for feveral years.

About five years ago, a little before which time the abovementioned periodical difcharge had difappeared, fhe had a fhort third epileptic fit, which was immediately fuc-
ceeded

ceeded by a fever of about a week's continuance, and of
which fhe recovered fo flowly that fhe had not been out
of doors till fix weeks after the crifis; when, without the
knowledge of her parents or any of the family (who
were all bufied in the harveft-field) fhe ftole out of the
houfe, and bound the corn of a ridge before they ob-
ferved her. On that fame evening fhe took to her bed
complaining much of her heart and head; and fince, fhe
has never rifen out of it except when lifted, has feldom
fpoken a word, and has had fo little craving for food
that at firft it was by downright compulfion her parent
could get her to take as much as would fupport a fuck-
ing infant: afterwards fhe gradually fell off from takin
even that fmall quantity; infomuch that, at Whitfun
tide 1763, fhe totally refufed food and drink, and her ja
became fo faft locked, that it was with the greateft diff
culty her father was able with a knife or other metho
to open her teeth fo as to admit a little thin gruel or whe
and of which fo much generally run out at the corne
of her mouth, that they could not be fenfible that a
of it had been fwallowed.

Much about this time, that is, about four years ag
they got a bottle of the water from a noted medicit
fpring in Brea-mar, of which they endeavoured to
her to fwallow a part, by pouring fome out of a fpo

between her lips (her jaws all the while faſt-locked) but it all run out. With this, however, they rubbed her throat and jaws, and continued the trial to make her ſwallow, rubbing her throat with the water that run out of her mouth for three mornings together. On the third morning during this operation, ſhe cried, Give me more water; when all that remained of the bottle was given her, which ſhe ſwallowed with eaſe. Theſe were the only words ſhe ſpoke for almoſt a year, and ſhe continued to mutter ſome more (which her parents underſtood) for twelve or fourteen days, after which ſhe ſpoke none, and rejected, as formerly, all ſorts of nouriſhment and drink, till ſome time in the month of July 1765, when a ſiſter of hers thought, by ſome ſigns that ſhe made, that ſhe wanted her jaws opened; which her father, not without violence, got done, by putting the handle of a horn-ſpoon between her teeth. She ſaid then intelligibly, Give me a drink; and drank with eaſe, and all at one draught, about an Engliſh pint of water. Her father then aſked her, Why ſhe would not make ſome ſigns, although ſhe could not ſpeak, when ſhe wanted a drink? She anſwered, why ſhould ſhe when ſhe had no deſire. At this period they kept the jaws aſunder with a bit of wood, imagining ſhe got her ſpeech by her jaws being opened, and continued them thus wedged for about twenty days,

though

though in the firſt four or five days ſhe had wholly loſt the power of utterance. At laſt they removed the wedge, as it gave her uneaſineſs, and made her lips ſore. At this time ſhe was ſenſible of every thing done or ſaid about her; and when her eye-lids were opened for her, ſhe knew every body; and when the neighbours in their viſits would be bemoaning her condition, they could ob-ſerve a tear ſtand in her eye.

In ſome of the attempts to open her jaws, two of the under fore-teeth were forced out; of which opening they often endeavoured to avail themſelves, by putting ſome thin nouriſhing drink into her mouth; but without effeǑt, for it always returned by the corners; and, about a twelvemonth ago, they thought of thruſting a little dough of oatmeal through this gap of the teeth, which ſhe would retain a few ſeconds, and then return with ſomething like a ſtraining to vomit, without one par-ticle going down: nor has the family been ſenſible, though obſerving, of any appearance like that of ſwallowing, for now four years, excepting the ſmall draught of Brea-mar water and the Engliſh pint of com-mon water; and for the laſt three years ſhe has not had any evacuation by ſtool or urine, except that, once or twice a week, ſhe has paſſed a few drops of urine, as the parents expreſs it, about as much as would wet the ſur-face.

face of a half-penny; and even fmall as this quantity is,
it gives her fome uneafinefs till fhe voids it: for they
know all her motions, and when they fee her thus un-
eafy, they carry her to the door of the houfe, where fhe
makes thefe few drops.   Nor have they, in all thefe
three years, ever difcovered the fmalleft wetting in her
bed; in proof of which, notwithftanding her being fo
long bed-ridden, there has never been the leaft excoria-
tion, though fhe never attempts to turn herfelf, or makes
any motion with hand, head, or foot, but lies like a log
of wood.   Her pulfe to-day, which with fome difficulty
I felt (her mother at this time having raifed her, and fup-
ported her in her bed) is diftinct and regular, flow, and
to the extremeft degree fmall. Her countenance is clear
and pretty frefh, her features not disfigured nor funk;
her fkin feels natural both as to touch and warmth; and
to my aftonifhment, when I came to examine her body,
for I expected to feel a fkeleton, I found her breafts
round, and prominent, like thofe of a healthy young
woman; her legs, arms, and thighs, not at all ema-
ciated; the *abdomen* fomewhat tumid, and the mufcles
tenfe; her knees bent, and her ham-ftrings tight as a
bow-ftring; her heels almoft clofe to the *nates.* When
they ftruggle with her, to put a little water within her
lips, they obferve fometimes a dewy foftnefs on her

<div align="right">fkin;</div>

ſkin; ſhe ſleeps much, and very quiet; but when awake keeps a conſtant whimpering like a new-born weakly infant, and ſometimes makes an effort to cough. At preſent no degree of ſtrength can force open her jaws. I put the point of my little finger into the gap in her teeth, and found the tongue, as far as I could reach, ſoft and moiſt; as I did with my other fingers the mouth and cheeks quite to the back teeth. She never can remain a moment on her back, but always falls to one ſide or to the other; and when her mother ſat behind her in the bed, and ſupported her while I was examining her body, her head hung down, with her chin cloſe to her breaſt, nor could I with any force move it backwards, the anterior muſcles of the neck being rigid, like a perſon in the *emproſthotonos*, and in this poſture ſhe conſtantly lies.

The above caſe was taken in writing this day, at the diſeaſed woman's bed-ſide, from the mouths of her father and mother, who are known to be people of great veracity, and are under no temptation to deceive; for they neither aſk, expect, or get any thing: their daughter's ſituation is a very great mortification to them, and univerſally known and regretted by all their neighbours. I had along with me, as interpreters (a), Mr. Henry Robertſon, a very diſcreet young gentleman, eldeſt ſon

(a) The family ſpoke only Erſe.

to the minifter of the parifh, and David Rofs, at the
Craig of Strath-Carron, their neighbour and one of the
elders of the parifh, who verified from his own know-
ledge all that is above related. The prefent fituation and
appearances of the patient were carefully examined this
21ft of October, 1767, by Dr. ALEXANDER MACKENZIE,
phyfician at New Tarbat; who likewife, in the month
of October, 1772, being informed that the patient was
recovering and ate and drank, vifited her, and found
her condition to be as follows: about a year preceding
this laft date, her parents one day returning from
their country labours (having left their daughter as
for fome years before fixed to her bed) were greatly
furprized to find her fitting on her hams, on the fide
of the houfe oppofite to her bed-place, fpinning
with her mother's diftaff. I afked, whether fhe ever
ate or drank? whether fhe had any of the natural
evacuations? whether fhe ever fpoke or attempted to
fpeak? And was anfwered, that fhe fometimes crumbled a
bit of oat or barley cake in the palm of her hand, as if to
feed a chicken; that fhe put little crumbs of this into the
gap of her teeth, rolled them about for fome time in
her mouth, and then fucked out of the palm of her hand
a little water, whey, or milk; and this once or twice a
day, and even that by compulfion: that the *egefta* were in-
proportion

proportion to the *ingesta*; that she never attempted to speak; that her jaws were still fast-locked, her hamstrings tight as before, and her eyes shut. On my opening her eye-lids I found the eye-balls turned up under the edge of the *os frontis*, her countenance ghastly, her complexion pale, her skin shrivelled and dry, and her whole person rather emaciated; her pulse with the utmost difficulty to be felt. She seemed sensible and tractable in every thing, except in taking food; for, at my request, she went through her different exercises, spinning on the distaff, and crawling about on her hams, by the wall of the house, with the help of her hands: but when she was desired to eat, she shewed the greatest reluctance, and indeed cried before she yielded; and this was no more than, as I have said, to take a few crumbs as to feed a bird, and to suck half a spoonful of milk from the palm of her hand. On the whole, her existence was little less wonderful now than when I first saw her, when she had not swallowed the smallest particle of food for years together. I attributed her thinness and wan complexion, that is the great change of her looks from what I had first seen when fixed to her bed, to her exhausting too much of the *saliva* by spinning flax on the distaff, and therefore recommended her being totally confined to spinning wool: this she does with equal dexterity

as fhe did the flax. The above was her fituation in
October, 1772; and within thefe eight days I have been
told by a neighbour of her father's, that fhe ftill con-
tinues in the fame way, without any addition to her fup-
port, and without any additional ailment.

New Tarbat,   ALEX. MACKENZIE.
April 3, 1775.

At Croick, the fifteenth Day of June, 1775.

TO authenticate the hiftory fet forth in the preceding
pages, DONALD MAC LEOD, of Granics efq. fheriff depute
of Rofs-fhire, GEORGE MUNRO efq. of Cuteain, SIMON
ROSS efq. of Gladfield, Captain GEORGE SUTHERLAND of
Elphin, all juftices of the peace; Meffieurs WILLIAM
SMITH preacher of the gofpel, JOHN BARCLAY writer in
Tain, HUGH ROSS ftudent of divinity, and ALEXAN-
DER MAC LEOD, did come to this place, accompanied by
the above Dr. ALEXANDER MACKENZIE phyfician at
New Tarbat, and after explaining the purport and mean-
ing of the above hiftory to DONALD MAC LEOD father to
JANET MAC LEOD above-mentioned, and to DAVID ROSS
elder in the parifh of Kincardin, who lives in the clofe
neighbourhood of this place, and was one of the doctor's
original interpreters; they, to our full fatisfaction, after
                                                    a minute

a minute examination, authenticate all the facts fet forth in the above account: and, for our further fatisfaction, we had JANET MAC LEOD brought out before us to the open air, when the doctor difcovered a very great improvement in her looks and health fince the period of his having feen her laft, as now fhe walked tolerably upright, with a little hold by the wall. And notwithftanding her age, which, upon inquiry, we found to be exactly as fet forth in the above account, her countenance and looks would have denoted her not to be above twenty years of age at moft. At prefent, the quantity of food fhe ufes is not above what would be neceffary for the fuftenance of an infant of two years of age. And we do report, from our knowledge of the above men, and the circumftances of the cafe, that full faith and credit is to be given to every article of the above hiftory.

WILLIAM SMITH.   DON^D. M^C LEOD, SH. DEP.
JOHN BARCLAY, N. P.   GEO. MUNRO, J. P.
HUGH ROSS.   SIMON ROSS, J. P.
ALEX^R. M^C LEOD.   GEO. SUTHERLAND, J. P.

II. *On the Ufefulnefs of wafhing and rubbing the Stems of*
   Trees, to promote their Annual Increafe.   In an Ex-
   tract of a Letter from Mr. Marfham *to the Lord Bifhop*
   of Bath *and* Wells.

I HAD for feveral years intended to put in
          practice the celebrated Dr. HALES advice
of wafhing, with that of Mr. EVELYN of rubbing the
ftem of a tree, in order to increafe its growth; but other
avocations prevented me till the laft fpring: when, as
foon as the buds began to fwell, I wafhed my tree round
from the ground to the beginning of the head; *viz.* be-
tween thirteen and fourteen feet in height. This was
done firft with water and a ftiff fhoe-brufh, until the
tree was quite cleared of the mofs and dirt; then I only
wafhed it with a coarfe flannel. I repeated the wafhing
three, four, or five times a week, during all the dry time
of the fpring and the fore-part of the fummer; but after
the rains were frequent, I very feldom wafhed. The un-
wafhed tree, whofe growth I propofed to compare with
it, was (at five feet from the ground) before the laft year's
increafe, 3 ft. 7 in. $\frac{9}{10}$ths; and in the autumn, after the

year's

year's growth was compleated, 3 ft. 9 in. $\frac{1}{10}$th; *viz.* increase 1 in. $\frac{2}{10}$ths. The washed tree was last spring 3 ft. 7 in. $\frac{2}{10}$ths, and in the autumn it was 3 ft. 9 in. $\frac{7}{10}$ths; *viz.* increase 2 in. $\frac{5}{10}$ths, that is, one-tenth of an inch above double the increase of the unwashed tree. As the difference was so great, and as some unknown accident might have injured the growth of the unwashed tree, I added the year's increase of five other beeches of the same age *(viz.* all that I had measured), and found the aggregate increase of the six unwashed beeches to be 9 in. $\frac{3}{10}$ths, which, divided by six, gives one inch and five-tenths and an half for the growth of each tree; so the gain by washing is nine-tenths and an half. To make the experiment fairly, I fixed on two of my largest beeches, sown in 1741, and transplanted into a grove in 1749. The washed tree had been, from the first year, the largest plant till the year 1767, when its rival became and continued the largest plant, until I began to wash the other: therefore I fixed on the less thriving tree as the fairest trial. The trees were nearly of the same height and shape, spreading a circle of about fifty feet diameter. I think it necessary to mention these circumstances; for I know by experience, that a short and spreading tree, having ample room, will increase twice or three times, and perhaps four times as much, as a tall small-headed

tree

tree of the fame age, that ftands near other trees. Thus my wafhed beech increafed above fix times as much as Mr. DRAKE's beautiful beech at Shardeloes, though that tree feemed in good health when I faw it in 1759 and 1766. But it increafed only 2 in. $\frac{9}{10}$ths in thofe feven years; which may perhaps be owing to its vaft height, being feventy-four feet and a half to the boughs (as the late knight of the fhire for Suffolk, Sir JOHN ROUS, told me that Mr. DRAKE had informed him) only fix feet and four inches round, and having a fmall head, and little room to fpread.

Stratton, Oct. 29, 1775.

III. *Discoveries on the Sex of Bees, explaining the Manner in which their Species is propagated; with an Account of the Utility that may be derived from those Discoveries by the actual Application of them to Practice.* By Mr. John Debraw, *Apothecary to* Addenbrook's *Hospital at* Cambridge, *and Member of an Oeconomical Society in the Principality of* Liege *in* Westphalia. *Communicated by the Rev.* Nevil Maskelyne, *B. D. F. R. S. and Astronomer Royal.*

Read Nov. 21, 1776.

THE republic of bees has at all times gained universal esteem and admiration: their culture, an object so worthy of our attention, has attracted and still does engage that of many of the learned, and has arrived at a considerable degree of improvement of late years; but their mode of propagating their species seems to this day to have baffled the ingenuity of ages in their attempts to discover it. The most skilful naturalists have been strangely misled in their opinion, that the bees, as well as the other tribes of animals, are perpetuated by copulation; though they acknowledge

acknowledge that they have never been able to detect them in the act.

PLINY, who was likewise of the same opinion, that in this particular they do not differ from other animals, observes, " *Apium coitus visus est nunquam.*" SWAMMER-DAM, that sagacious observer, having never been able to discover it, entertained a notion, that the female or queen bee was fecundated without copulation; that it was sufficient for her to be near the males; that a vivifying *aura*, exhaling from the body of the males, and absorbed by the female, might impregnate her eggs. At last the incomparable REAUMUR thought he had in a great measure removed the veil, and brought their manner of generating nearly to a proof. This part of physics has been the principal object of my researches for several years past, having been insensibly engaged in it by the pleasure I took in so curious an inquiry; and although this pursuit has been attended with more difficulties and embarassments than can be well imagined, I have not been discouraged, and have carefully avoided launching into conjectures. To introduce a new system in the doctrine of bees, which in a great measure contradicts all former received opinions, requires, previous to its appearance, every sanction the various experiments, successfully repeated, can possibly give it. The results of those experi-

ments,

ments, made all in glafs-hives, which carry with them an entire evidence, afford fufficient reafons to affert, that bees belong to that clafs of animals among which, although they have fexes, a true copulation cannot be proved; and that their *ova*, like the fpawn of fifhes, moft probably owe their fecundation to an impregnation from the males, as will appear in the fequel of this narrative.

I am not a little pleafed to find that the celebrated MA-RALDI had fuch a notion, and I lament his neglecting to confirm it. He fays, in his *Obfervations upon Bees*, in the Hiftory of the Academy of Sciences for the year 1712, p. 332: *Nous n'avons pû découvrir jufqu' à prefent de quelle maniere fe fait cette fécondation, fi c'eft dans le corps de la femelle, ou bien fi c'eft à la maniere des poiffons, aprés que la femelle a pofé fes œufs : la matiere blanchâtre dont l'œuf eft environné au fond de l'alvéole peu de temps aprés fa naiffance, femble conforme à la derniere opinion, auffi-bien que les remarques faites plufieurs fois d'un grand nombre d'œufs qui font reftés inféconds au fond de l'al-véole autour defquels nous n'avons point vû cette matiere.* " We never yet were able to difcover in what manner " this fecundation is performed; whether it is in the body " of the female, or whether it is after the manner of " fifhes, after the female or queen-bee has depofited " her eggs; that liquid whitifh fubftance, with which

" each

" each egg is furrounded at the bottom of the cell a
" little while after its being laid, feemingly eftablifhing
" this laft opinion, as well as the frequent remarks made
" of a great number of eggs remaining barren in the cell,
" round which we could not fee the above-mentioned
" whitifh fubftance."

· This ingenious naturalift, by a nice examination of
the ftructure of the drones, had, as well as SWAMMER-
DAM, difcovered fome refemblances to the male organs
of generation; and from thence conjectured, they were
the males of the bee-infect; but he owns, with the reft,
that he never could difcover them in the act of copu-
lation.

· Having ftood the trials of fo many prying eyes in
every age, the bees, as has been obferved by an ingenious
author, had gained the character of an inviolable chaftity,
till REAUMUR blafted their reputation. He makes the
queen no better than a MESSALINA[a]; though he could
fee no more than what would raife a mere jealoufy or
generate fufpicions.

In order to be the better underftood in the relation of
my own experiments on the fecundation of bees, I here
premife the outlines of the opinions adopted by the above-
mentioned naturalifts on that head. They affert that the

[a] Vid. JUVENAL, Sat. vi. ver. 128.

queen

queen is the only female in the hive, and the mother of the next generation; that the drones are the males by which fhe is fecundated; and that the working bees, or bees that collect wax on the flowers, that knead it and form from it the combs and cells which they afterwards fill with honey, are of neither fex.

But of late Mr. SCHIRACH, a German naturalift, has given us a very different view of the claffes that conftitute the republic of bees, in an ingenious publication in his own language, under the title of *The Natural Hiftory of the Queen of the Bees,* which has been fince tranflated into French; an account of which has been given in the Monthly Review; from which I beg leave to relate the author's doctrine with regard to the working-bees only; the quality and functions of the drones being points which do not appear to be yet fettled by Mr. SCHIRACH himfelf. He affirms, that all the common bees are females in difguife, in which the organs that diftinguifh the fex, and particularly the *ovaria,* are obliterated, or at leaft, through their exceffive minutenefs, have not yet been obferved: that every one of thofe bees in the earlier period of its exiftence is capable of becoming a queen-bee, if the whole community fhould think proper to nurfe it in a particular manner, and raife it to that rank. In

fhort,

fhort, that the queen-bee lays only two kinds of eggs;
viz. thofe that are to produce the drones, and thofe from
which the working-bees are to proceed.

The trials made by Mr. SCHIRACH feem to evince the
truth of his conclufions in the moft fatisfactory manner,
fingular as they appear to be at firft fight; and indeed
in my own judgement, from the conftant happy refult
of my numerous experiments, which I began near two
years before Mr. SCHIRACH's publication, and repeated
every feafon fince, I am enabled to pronounce on their
reality.

Chance I own befriended me in that difcovery, whilft
I was moft anxioufly endeavouring to afcertain the ufe of
drones. It was in the fpring of the year 1770, that I for
the firft time difcovered what MARALDI had only con-
jectured, I mean the impregnation of the eggs by the
males, and that I was made acquainted with the difference
of fize in the drones or males obferved by MARALDI in his
Obfervations upon Bees, inferted in the Hiftory of the Royal
Academy of Sciences for the year 1712, p. 333. in thefe
words:

Nous avons trouvé depuis peu une grande quantité de
bourdons, beaucoup plus petits que ceux que nous avions
remarqué auparavant, et qui ne furpaffent point la gran-
deur des petites abeilles; de forte qu'il n'auroit pas été aifé

de les diſtinguer dans cette ruche de abeilles ordinaires, ſans le grand nombre que nous y en avons trouvé. Il ſe pourroit bien faire que dans les ruches où l'on n'a pas trouvé de gros bourdons, il y en eût de ces petits, et qu'ils y aient été con-fondus avec le reſte des abeilles, lorſque nous ne ſavions pas encore qu'il y en eût de cette taille. "We have of "late found a great quantity of drones much ſmaller "than thoſe we had formerly obſerved, and which do not "exceed in ſize the common bees; ſo that it would not "have been eaſy to diſtinguiſh them in that hive from "the common bees, had not the quantity of them been "very conſiderable. It might certainly have happened "that in thoſe hives, where we have not been able to "diſcover large drones, there were a great number of "thoſe little ones, which may have been intermixed "among common bees when we were yet ignorant that "any ſuch ſmall drones were exiſting."

REAUMUR himſelf, p. 591. of his Natural Hiſtory of Inſects, ſays, "We have likewiſe found drones that were "no bigger than the common bees."

They have notwithſtanding eſcaped the obſervation of Mr. SCHIRACH, and of his friend Mr. HATTORF member of an Academy in Luſatia, who, in a memoir he preſented in the year 1769, annihilates entirely the uſe of drones in a hive; and advances this ſingular opinion,

opinion, that the queen-bee of a hive lays eggs which produce young ones, without having any communication with the drones. For what purpofe fhould wife nature then have furnifhed the drones with that large quantity of feminal liquor? To what ufe fo large an apparatus of fe-cundating organs, fo well defcribed by REAUMUR and MARALDI?

But I beg leave to remark, that thofe gentlemen feem to have drawn too hafty conclufions from their experiments, in rejecting the drones as bearing no fhare in the propaga-tion of thofe infects. Their obfervations, that hives are peopled at a time of the year when there are no drones in being, is no ways conclufive; as it is evident, that they had feen none but drones of a large fize, their filence on the difference in the fize of them juftifying my remark. But to refume the narrative of my experiments: I had watched my glafs-hives(b) with indefatigable attention, from the moment the bees, among which I had taken care to leave a large number of drones, were put into them, to the time of the queen laying her eggs, which gene-rally happens the fourth or fifth day. I obferved the firft or fecond day (always before the third) from the time the eggs are placed in the cells, that a great number

(b) Glafs-hives were ufed in preference to boxes, for a purpofe too obvious to need explaining.

of

of bees, faftening themfelves to one another, hung down
in the form of a curtain from the top to the bottom of the
hive, in a fimilar manner they had done before at the
time the queen depofited her eggs; an operation which
(if we may conjecture at the inftincts of infects) feems
contrived to hide what is tranfacting: be that as it will,
it anfwered the purpofe of informing me that fome-
thing was going forward. In fact, I prefently after per-
ceived feveral bees, the fize of which through this thick
veil (if I may fo exprefs myfelf) I could not rightly dif-
tinguifh, inferting the pofterior part of their bodies each
into a cell, and finking into it, where they continued but
a little while. After they had retired, I faw plainly with
the naked eye a fmall quantity of a whitifh liquor left in
the angle of the bafis of each cell, containing an egg: it
was lefs liquid than honey, and had no fweet tafte at all.
Within a day after, I found this liquor abforbed into the
embrio, which on the fourth day is converted into a fmall
worm, to which the working-bees bring a little honey
for nourifhment, during the firft eight or ten days after
its birth. After that time they ceafe to feed them; for
they fhut up the cells, where thefe embrios continue in-
clofed for ten days more, during which time they un-
dergo various changes too tedious here to defcribe.

To

To evince the reality of this obfervation, and to prove
that the eggs are fecundated by the males, and that their
prefence is neceffary at the time of breeding, I proceeded
to the next experiments. They confifted in leaving in a
hive the queen with only the common bees, without any
drones, to fee whether the eggs fhe laid would be pro-
lific.   I accordingly took a fwarm, fhook all the bees
into a tub of water, and left them in it till they were
quite fenfelefs, which gave me an opportunity to dif-
tinguifh the drones without any danger of being ftung.
After I had recovered the working-bees and their queen
from the ftate they were in, by fpreading them on
brown paper in the fun, I replaced them in a glafs-hive,
where they foon began to work as ufual: the queen
laid eggs, which I little fufpected to be impregnated,
as I thought I had feparated all the drones or males, and
therefore omitted watching the bees; but at the end of
twenty days (the ufual time of their hatching) I found
to my furprize fome of the eggs hatched into bees, others
withered away, and feveral of them covered with honey.
I immediately inferred that fome of the males, having
efcaped my notice, had impregnated only part of the
eggs; but, in order to convince myfelf of the truth of
my fuppofition, I thought it neceffary to take away all
the brood-comb that was in the hive, in order to oblige

the

the bees to provide a fresh quantity, being fully determined to watch narrowly their motions after new eggs should be deposited in the cells. This was done accordingly, and at last the mystery was unravelled. On the second day after the eggs were placed in the cells, I perceived the same operation which I have related in a former experiment; I mean, the bees hung down in the form of a curtain, while others thrust the posterior part of their body into the cells: I then introduced my hand into the hive, broke off a piece of the comb containing two of those insects, and kept them for examination. I found in neither of them any sting (a circumstance peculiar to drones only) and upon dissection, by the help of a DOLLOND's microscope, discovered in them the four cylindrical bodies, which contain the glutinous liquor of a whitish colour, observed by MARALDI in the large drones.

Having till then never observed any difference in the size of drones, I immediately perused the Memoirs on Bees published by Messr. MARALDI and REAUMUR, and found that they had remarked it frequently. I have inserted in a preceding page the substance of their observations on that head, as taken from their writings. The reason of that difference must I doubt be placed amongst other *arcana* of nature. I found myself therefore under a necessity in my next experiments to be more particular

in deftroying the males, even thofe which might be fuf-
pected to be fuch.

I once more immerfed all the fame bees in water; and,
when they appeared to be in a fenfelefs ftate, I gently
preffed every one of them between my fingers, in order
to diftinguifh thofe armed with ftings from thofe that
had none, which laft I might fufpect to be males. Of
thefe I found fifty-feven, exactly of the fize of common
bees, yielding a little whitifh liquor on being preffed be-
tween the fingers. I killed every one, and replaced the
fwarm in a glafs-hive, where they immediately applied
again to the work of making cells; and on the fourth
or fifth day, very early in the morning, I had the plea-
fure to fee the queen-bee depofiting her eggs in thofe
cells, which fhe did by placing the pofterior part of her
body in each of them. I continued on the watch moft
part of the enfuing days, but could difcover nothing of
what I had feen before.

The eggs, after the fourth day, inftead of changing in
the manner of caterpillars, were found in the fame ftate
they were in the firft day, except that fome of them were
covered with honey. But a very fingular event happened
the next day about noon: all the bees left their own hive,
and were feen attempting to get into a neighbouring com-
mon hive, on the ftool of which I found their queen dead,

     having

having no doubt been flain in the engagement. The manner in which I account for this event is as follows: the great defire of perpetuating their fpecies, which is moft obfervable in thefe infects, and to which end the concurrence of the males feems fo abfolutely neceffary, made them defert their own habitation where no males were left, in order to fix their refidence in a new one, in which, there being a good ftock of males, they might the better accomplifh their purpofe. If this does not yet eftablifh the reader's faith of the neceffity of the males bearing a fhare in the fecundation of the *ova*, the next experiment cannot I prefume fail to convince him.

I took the brood-comb which, as I obferved before, had not been impregnated; I divided it into two parts; one I placed under a glafs-bell N° 1. with honey-comb for the bees' food; I took care to leave a queen, but no drones, among the common bees I confined in it. The other piece of brood-comb I placed under another glafs-bell N° 2. with a few drones, a queen, and a number of common bees proportioned to the fize of the glafs; the reft I dif-pofed of as before. The refult was, that in the glafs N° 1. no impregnation happened; the eggs remained in the fame ftate they were in when put into the glafs; and, upon giving the bees their liberty on the feventh day, they all flew away, as was found to be the cafe in the for-

mer experiment: whereas in the glafs N° 2. I faw, the
very day after the bees had been put under it, the impreg-
nation of the eggs by the drones in every cell containing
eggs; the bees did not leave their hive on receiving their
liberty; and, in the courfe of twenty days, every egg un-
derwent all the above-mentioned neceffary changes, and
formed a pretty numerous young colony, in which I was
not a little ftartled to find two queens.

Fully fatisfied concerning the impregnation of the eggs
by the males, I defifted for the prefent from any further
experiments on that head, being exceedingly anxious to
endeavour to account for the prefence of this new queen.

I conjectured that either two queens, inftead of one,
muft have been left among the bees I had placed under
that glafs; or elfe that the bees could, by fome particular
means of their own, transform a common fubject into a
queen.

In order to put this to the teft, I repeated the experi-
ment with fome variation. I got four glafs-hives blown
flat, which I thought preferable to the bell-fhaped ones
I had ufed before, as I could with thofe better examine
what was going forward. I took a large brood-comb
from an old hive, and, after having divided it into feveral
pieces, I put fome of them, containing eggs, worms, and
nymphs, with food, *viz.* honey &c. under each of the

glaffes; and confined within each a fufficient number of common bees, among which I left fome drones, but took care that there fhould be no queen.

The bees finding themfelves without a queen, made a ftrange buzzing noife, which lafted near two days; at the end of which they fettled and betook themfelves to work: on the fourth day I perceived in each hive the beginning of a royal cell, *a certain indication that one of the inclofed worms would foon be converted into a queen.* The conftruction of the royal cell being nearly accomplifhed, I ventured to leave an opening for the bees to get out, and found that they returned as regularly as they do in common hives, and fhewed no inclination to defert their habitation. But, to be brief, at the end of twenty days, I obferved four young queens among the new progeny.

On relating the refult of thefe experiments to a member of this univerfity, well converfant in the natural hiftory of bees, he deemed it neceffary, that they fhould be repeated, in order the better to eftablifh the truth of a fact feemingly fo improbable, that the eggs deftined by nature to produce neutral or common bees, fhould be transformed into females or queens. He ftarted an objection to me, which by the publication of Mr. SCHIRACH appearing a little time after, feems to have been pointed out to that author alfo by Mr. WITHELMI,

his

his brother-in-law, namely, that the queen-bee of a hive, befides the eggs which fhe depofits in the royal cells, might alfo have laid royal or female eggs either in the common cells, or indifcriminately throughout the different parts of the hive. He further fuppofed, that in the pieces of brood-comb, which had been fuccefsfully employed in the laft experiments for the production of a queen, it had conftantly happened, that one or more of thefe royal eggs, or rather the worms proceeding from them, had been contained.

But the force of his objection was removed foon after by the fame fuccefs having attended a number of other experiments which I fince made, an account of which would take up too much room here; and this gentleman, together with Mr. SCHIRACH's brother-in-law, was at laft brought to admit, that the working-bees are invefted with a power of raifing a common fubject to the throne, when the community ftands in need of a queen; and that accordingly every worm of the hive is capable, under certain circumftances, of becoming the mother of a generation: that it owes its metamorphofis into a queen, partly to the extraordinary fize of the cell, and its particular pofition in it; but principally to a certain nourifhment appropriated to the occafion, and carefully adminiftered to it by the working-bees while it is in the worm-ftate,

by

by which, and poffibly other means as yet unknown, the developement and expanfion of the germ of the female organs, previoufly exifting in the embryos, is effected, and thofe differences in its form and fize are produced, which afterwards fo remarkably diftinguifh the queen from the common working-bees. And finally it appears evident, from the experiments made by Mr. SCHIRACH and myfelf, that the received opinion, that the queen lays a particular kind of eggs, appropriated to the production of other queens, is erroneous. I am not a little flattered with the fimilarity of my difcoveries with thofe of the ingenious German naturalift, in proving the fex of the common bees; although we fo widely differ in what relates to the ufe of the males, whom, as we have feen before, he imagines to be quite ufelefs. I am alfo not a little pleafed to find, that our experiments on the production of a queen from a common embryo agree fo well.

I fhall now beg leave to point out the advantage that may accrue to the public from thefe obfervations, which is that of forming artificial fwarms or new colonies; or in other words, of furnifhing the means to bring on a numerous increafe of thofe ufeful infects: an object of fome importance to this kingdom, as being the only means to prevent the annual exportation of confiderable fums in the purchafe of wax, a great quantity

of

of which is left every feafon for want of keeping up a fufficient ftock of bees to collect it.

The practice of this new art, Mr. SCHIRACH tells us, has already extended itfelf through Upper Lufatia, the Palatinate, Bohemia, Bavaria, Silefia, and even in Poland. In fome of thofe countries it has excited the attention and patronage of government; and even the Emprefs of Ruffia has thought it of fuch importance, that fhe has fent a perfon to Klein Bautzen, to be inftructed in the general principles, and learn all the *minutiæ* of this new art.

The narrow limits of this paper do not permit me here to give an account of Mr. SCHIRACH's ingenious ob-fervations. I beg leave to refer the curious reader to the work itfelf, which, with the reviewers, I wifh was tranf-lated into the Englifh language, as it contains many par-ticulars highly deferving the notice of the fpeculative naturalift, as well as of thofe who cultivate bees either for profit or amufement.

IV. *An Account of a Portrait of* Copernicus, *prefented to the* Royal Society *by Dr.* Wolf *of* Dantzick: *extracted from a Letter of his to Mr.* Magellan, *F. R. S.*

DEAR SIR,

Read Dec. 7, 1776.

THE captain who will deliver this to you, will alfo put into your hands a copy of an original portrait of the famous COPERNICUS, which I beg you will prefent to the Royal Society, as a teftimony of my devotion and attachment to that refpectable body. The original, from which it is copied with the greateft accuracy, is in the poffeffion of the Chamberlain HUS-

SARZEWSKI,

Avis touchant un Portrait de Copernic, *préfenté à la* Societé Royale, *par M. le Dr.* Wolf *de* Danzic: *extrait d'une Lettre du dit Dr. à M. de* Magellan, *Membre de la Soc. R.*

Monfieur et très cher ami,   Danzic, le 7 Avril, 1776.

LE capitaine qui vous délivrera celle-ci, vous apportera en même tems la copie d'un portrait du fameux COPERNIC, que je vous prie de préfenter à votre illuftre Societé Royale, comme un temoignage de mon dévouement et refpect pour cet illuftre Corps. Le poffeffeur de l'original, dont le portrait fut copié avec la plus grand attention, eft M. le Chambellan HUSSARZEWSKI. Il a déja

SARZEWSKI, who has already refufed one hundred ducats for it, and will not part with it at any rate during his life, but intends to bequeath it me after his death; for which reafon there is no probability of my ever poffeffing it, as he is likely to furvive me many years. We have a portrait of COPERNICUS in the great church at Thorn in a kind of maufoleum, erected about thirty years after the death of that great man, by a phyfician of that town, who is faid to have been one of his relations.

HARTKNOCH has inferted a print taken from this portrait in his Chronicles of Pruffia. Our original has been compared with that of the maufoleum, and the features of the face are found to be perfectly fimilar, but there is a great difference in the drefs. That at Thorn reprefents him kneeling before an altar, in the attitude of a prieft officiating; in ours he is cloathed in fur, with his hair

more

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déja refufé 100 ducats, et ne le cédera pas fa vie durant pour aucun prix : mais il me le veut léguer après fa morte. C'eft à dire très probablement après la mienne : de forte qu'il n'y a rien à efpérer de ce coté-ci. Nous avons un portrait de COPERNIC dans la grande Eglife de Thorn, dans une efpéce de maufolée, érigé une trentaine d'années après la mort de cet homme célebre, par un médecin de la ville, qu'on dit avoir été un de fes parents.

HARTKNOCH a fait graver ce portrait, et l'a inferé dans fa Chronique de la Pruffe. Notre original a été comparé fur le lieu avec celui du maufolée, et on a trouvé les traits du vifage abfolument les mêmes : mais l'habillement eft très different. Celui de Thorn le reprefente devant l'autel en fonction de prêtre.

Dans

more carefully dreffed, and as it were in a habit of ceremony. The painter of it was certainly one of the old Italians, as will appear by comparing it with other works of thofe mafters; for inftance, it is known that the painters of thofe times, and even RAPHAEL, never gave to the eyes that brightnefs which the moft indifferent artifts within this century never fail to exprefs in their portraits: not but what the ferene and feemingly inanimated countenances of the former artifts came nearer to nature than the fparkling eyes which are now all the fafhion. This however is a proof that the portrait is at leaft one hundred and fifty years old; the infcription fhews that the painter was an Italian; and it muft further be obferved, that it is now two centuries fince they left off painting on wood.

The

Dans le notre il eft habillé en péliffe, avec les cheveux de la tête plus foigneufement arrangés et coupés, comme en habit de parade. Le pinceau eft fûrement Italien du vieux tems en queftion, comme on trouvera en regardant d'autres de ce tems et les comparant avec celui-ci. Par exemple, on fait que les peintres dans ce vieux tems, même RAPHAEL, ne favoient pas encore donner aux yeux cette vivacité, que le plus médiocre peintre depuis plus d'un fiecle, ne manque pas de donner à fes peintures. Je ne dis pas que les vieux peintres n'approchent pas plus de la nature avec leurs yeux tranquiles fans feu; mais ils déplaifent à nôtre fiecle, qui ne veut que des yeux petillants. Au moins c'eft la marque d'une ancienneté plus grande qu'un fiecle et demi. L'infcription fait voir que le peintre etoit Italien. Il y a deux fiecles qu'on a ceffé de peindre fur du bois.

L'hiftoire

The hiſtory of this portrait is as follows. It was formerly in the collection of Saxe Gotha, where it was always conſidered as an original, which is even ſaid to appear from the archives of that court, and is the more probable, as the prince-biſhop of Warmia, who obtained it from the late duke of Saxe Gotha, was too good a connoiſſeur and too cautious to be deceived in this reſpect. That biſhop being at Gotha in the year 1735, obſerved this portrait in the gallery of that palace; the proofs that were produced of its authenticity made him very deſirous to acquire it. He at length obtained it by a kind of theft which it was neceſſary to commit on the cathedral of Warmia, in which there was a very old portrait of one of the anceſtors of the dukes of Saxe Gotha, who had been biſhop of that ſee, and whoſe picture was wanting

in

L'hiſtoire de notre original eſt la ſuivante. Il a été conſervé dans la collection des ducs de Saxe Gotha, dans le chateau du même nom, toujours avoué comme un véritable original: on prétend même prouvé comme tel par les archives; ce qui eſt fort croyable, vû que le prince evêque de Warmie, qui l'a obtenû du duc defunt, etoit trop circonſpect et trop bon connoiſſeur pour s'en laiſſer impoſer. Enfin c'eſt après les paroles du prince evêque, que je fais cette aſſertion. Ce fut environ l'année 1735 qu' etant à Gotha, il remarqua ce portrait dans la gallerie du duc. Les preuves qu'on lui fit de ſon authenticité, augmenterent ſon envie de le poſſéder. Il l'obtint enfin par un vol qu'il fallut faire à la Cathédrale de Warmie, où ſe trouvoit un très ancien portrait d'un des ancétres du duc de Saxe Gotha, qui etoit jadis evêque de Warmie, et dont le portrait manquoit dans la collection des portraits de ſa famille. On fit donc un échange.

in the duke's collection of the portraits of his family. An exchange was accordingly made of the two originals, and the bishop has since bequeathed that of COPERNICUS to his favourite Mr. HUSSARZEWSKI.

The copy I herewith send you is very exact in every respect, except that I have glued three wooden lists on the back of the board to prevent its warping. After my copy was taken, the proprietor of the original thought proper to have it repaired and varnished, and they are now so much alike that it is scarce possible to discern any difference. The name of my painter is LORMAN of Berlin, an artist of some reputation.

échange des deux originaux. Le prince evêque GRABOWSKI enfin a laissé celui de COPERNIC à son favori M. HUSSARZEWSKI.

La copie est absolument la même que l'original : excepté que j'ai fait coller trois régles sur le derriere de la table pour empêcher son courbissement. Depuis que ma copie a été tirée, M. HUSSARZEWSKI, croiant bien faire, a fait renouveller son original, c'est à dire, y mettre du vernis : et actuellement il est si bien le même que la copie, qu'on peut a peine s'appercevoir de la moindre différence. Mon peintre est M. LORMAN de Berlin, assez célebre dans son art.

V. *An Account of a Journey into* Africa *from the Cape of* Good-Hope, *and a Description of a new Species of* Cuckow. *By Dr.* Andreas Sparrman, *of the Royal Academy of* Stockholm, *in a Letter to Dr.* John Reinhold Forster, *F. R. S.*

DEAR SIR,   Gottenburg.
                                                   Sept. 16, 1776.

Read Dec. 19, **B**EING returned to my native country
    1776.   after an absence of five years from it,
I will endeavour to give you a short account of my expedition into Africa, which I undertook soon after parting with you at the Cape of Good-Hope. The voyage round the world, of which I shared the perils and pleasures with you, had only made me more eager to continue my rambles in quest of new discoveries. I set out therefore from the neighbourhood of the Cape-town in the beginning of August 1775, with no other company than the son of the Dutch lieutenant EMELMAN, who had formerly accompanied my learned friend Dr. THUNBERG on a similar journey, and some Hottentots who took care of my oxen.

I   The

The firſt misfortune I met with was the loſs of the thermometer which you had left me, and which broke before I had reached the hot-baths. This was only a prelude to greater diſtreſſes. The drought was ſo violent this year, that the like had not been experienced in the colony within the memory of man, and it obliged the inhabitants to leave their country-ſeats. A great part of their cattle periſhed for want of graſs and water, and I have frequently ſuffered the moſt raging thirſt in the hot defarts which I traverſed; but I was too well ſeaſoned during the voyage to dread the hardſhips of a ſcanty ſubſiſtence, the fatigues of travelling, or the power of the climate. The moſt ſenſible misfortune which the dry ſeaſon brought along with it, was the deſolation of the vegetable kingdom. Far from being ſo fortunate as Dr. THUNBERG, who has added above a thouſand ſpecies to the *Flora Capenſis*, I found every thing burnt up, and only in the thickeſt foreſts met with ſome perennial plants which were new to me, and which, upon a reviſal of that gentleman's herbal, I believe are likewiſe unknown to him. Of theſe I propoſe to ſend you ſpecimens as ſoon as I can find time to bring my collection into ſome kind of order. On the other hand, I have been fortunate with animals, and eſpecially in the claſs of quadrupeds. I ſhall not ſpeak of lions and other beaſts

of

of prey, which I have frequently feen in broad day-light, and heard roaring about me at night, though they never ventured to attack our cattle. But it was chiefly among the antelopes and animals of that fort that I hunted. Mr. EMELMAN and myfelf, with nine hottentots, a waggon drawn by feveral pairs of oxen, and feveral hunting horfes, happily traverfed a defart of fifty miles, where we had greater fport than any German prince could ever boaft of. On that route I penetrated farther into the country than any of my predeceffors, having gone one hundred miles beyond the laft Chriftian's or Dutchman's hut, into the diftrict of the Yellow or (as they are vulgarly called) Chinefe Hottentots.

The great buffaloes which inhabit the wilds of Africa, do not appear to me to differ in any refpect from the North American *Bifon*, although I have feen great numbers of them. I have likewife found a fpecies of pole-cat on that continent which LINNÆUS calls *Viverra Putorius*, contrary to M. DE BUFFON's opinion, who feems to confine this animal and its fpecies to America. By the fea-fide I was fortunate enough to catch a *Manatee* alive, notwithftanding the difficulty which muft attend the capture of fuch an unweildy animal. There I likewife faw fome iflands, on which I was told an Englifh fhip had been loft. Thefe I fufpected at firft to be the *Doddingtons*; but

afterwards

afterwards had reafon to doubt it, thofe iflands being fuppofed to lie in a more foutherly latitude.

I have had opportunities of making many curious and valuable Obfervations relative to the different tribes of Hottentots, their oeconomy, hunting-matches, and other cuftoms; an account of which, together with fome remarks on the natural hiftory of the elephant, the rhinoceros, and other animals, I intend to prepare for the prefs. I am poffeffed of an accurate map of that part of Africa which I have vifited, containing all the hills, together with the fmalleft rivulets, as far as the Bay de la Goa, which, I think, will be a great addition to the work. I only regret that I was not able to draw the objects of natural hiftory, and have an hundred times wifhed that your fon had been with me for this purpofe.

As I had been upwards of nine months on this journey, at my return to the Cape I found that my acquaintance had given up all hopes of feeing me again, having had no tidings of me for fo long a fpace of time. Notwithftanding the many dangers to which I had been expofed on this expedition, I affure you I was greatly tempted to ftay another year, in hopes of being more fuccefsful in botanical difcoveries. However, the profpect of fecuring the fpoils which I had collected, prevailed on me to relinquifh that fcheme. Indeed I little thought at that time that the greateft

danger awaited my collection in Sweden. A few days ago a great part of it has been damaged here by fire, which has been particularly fatal to my stuffed birds, having destroyed some which were not yet described.

As I am well acquainted with the pleasure which every new discovery in the history of nature gives you, I take this early opportunity of expressing the readiness with which I wish to contribute to your satisfaction, and have subjoined to this letter an account of a curious bird, a species of Cuckow, which I have saved out of the fire. I only beg that you will consider it as an earnest of more important communications, as soon as the hurry of my affairs will permit me to bring my papers into order. In the mean time if you should think that account, and the annexed drawing, worthy the attention of the Royal Society, I should be greatly flattered if you would do me the honour to lay it before that learned Body.

With the greatest esteem I remain, &c.

THE

THE HISTORY OF THE HONEY-GUIDE, or CUCULUS INDICATOR.

THIS curious fpecies of Cuckow is found at a confiderable diftance from the Cape of Good-Hope, in the interior parts of Africa, being entirely unknown at that fettlement. The firft place where I heard of it was in a wood, called the *Groot Vaader's Bofch*, the Grand Father's Wood, fituated in a defart near the river which the Hottentots call *T'kaut'kai*. The Dutch fettlers thereabouts have given this bird the name of *Honig-wyzer*, or Honey-guide, from its quality of difcovering wild-honey to travellers. Its colour has nothing ftriking or beautiful, as will appear from the defcription and drawing annexed; and its fize is confiderably fmaller than that of our Cuckow in Europe: but in return, the inftinct which prompts it to feek its food in a fingular manner, is truly admirable. Not only the Dutch and Hottentots, but likewife a fpecies of quadruped, which the Dutch name a *Ratel* (a), are frequently conducted to wild bee-hives by this bird, which as it were pilots them to the very fpot. The honey being its favourite food, its own intereft prompts it to be inftrumental in robbing the hive, as

(a) Probably a new fpecies of badger.

fome

fome fcraps are commonly left for its fupport. The morn-
ing and evening are its times of feeding, and it is then
heard calling in a fhrill tone *cherr*, *cherr*, which the honey-
hunters carefully attend to as the fummons to the chace.
From time to time they anfwer with a foft whiftle, which
the bird hearing, always continues its note. As foon as
they are in fight of each other, the bird gradually flut-
ters towards the place where the hive is fituated, conti-
nually repeating its former call of *cherr*, *cherr:* nay, if it
fhould happen to have gained a confiderable way before
the men (who may eafily be hindered in the purfuit by
bufhes, rivers, and the like) it returns to them again, and
redoubles its note, as if to reproach them with their in-
activity. At laft the bird is obferved to hover for a few
moments over a certain fpot, and then filently retiring to
a neighbouring bufh or other refting-place, the hunters
are fure of finding the bees neft in that identical fpot,
whether it be in a tree, or in the crevice of a rock, or (as
is moft commonly the cafe) in the earth. Whilft the
hunters are bufy in taking the honey, the bird is feen
looking on attentively to what is going forward, and wait-
ing for its fhare of the fpoil. The bee-hunters never fail
to leave a fmall portion for their conductor, but commonly
take care not to leave fo much as would fatisfy its hunger.
The bird's appetite being only whetted by this parfimony,

it

it is obliged to commit a fecond treafon, by difcovering another bees-neft, in hopes of a better falary. It is further obferved, that the nearer the bird approaches the hidden hive, the more frequently it repeats its call, and feems more impatient.

I have had frequent opportunities of feeing this bird, and have been witnefs of the deftruction of feveral republicks of bees, by means of its treachery. I had however but two opportunities of fhooting it, which I did to the great indignation of my Hottentots. From thofe fpecimens (both of which are fuppofed to be females) I have made the fubfequent defcription. The inhabitants in general accufe the fame bird of fometimes conducting its followers where wild beafts and venomous ferpents have their places of abode: this however I never had an opportunity of afcertaining myfelf; but am apt to believe fuch cafes to be accidental, when dangerous animals happen to be in the neighbourhood of a bees-neft.

Whilft I ftaid in the interior parts of Africa, a neft was fhewn to me, which fome peafants affured me was the neft of a Honey-guide. It was woven of flender filaments or fibres of bark, in the form of a bottle. The neck and opening hung downwards, and a ftring in an arched

shape was suspended across the opening, fastened by the
two ends, perhaps for the bird to perch upon.

DESCRIPTIO CUCULI INDICATORIS.

ROSTRUM crassiusculum, versus basin fuscum, apice
luteum.

> *Angulus oris* usque infra oculos extensus.

> *Nares* postremæ ad basin rostri, supremæ vicinæ
> ut carinulâ dorsali saltem separerentur, oblongæ,
> margine prominulo.

> *Pili* aliquot ad basin rostri, præcipuè in mandibulâ
> inferiore.

> *Lingua* plana, subsagittata.

Oculorum irides ferrugineo-grifeæ.

Palpebræ nudæ, nigræ.

Pedes nigri, scansorii. *Tibiæ* breves; *Ungues* tenues, nigri.

Pileus læte griseus e pennis brevibus latiusculis.

Gula, Jugulum, Pectus, sordidè alba, cum aliquo virore
vix notabili in pectore.

Dorsum et *Uropygium* ferrugineo-grisea.

Abdomen, Crissumque alba.

Femora tecta pennis albis, macula longitudinali nigra
notatis.
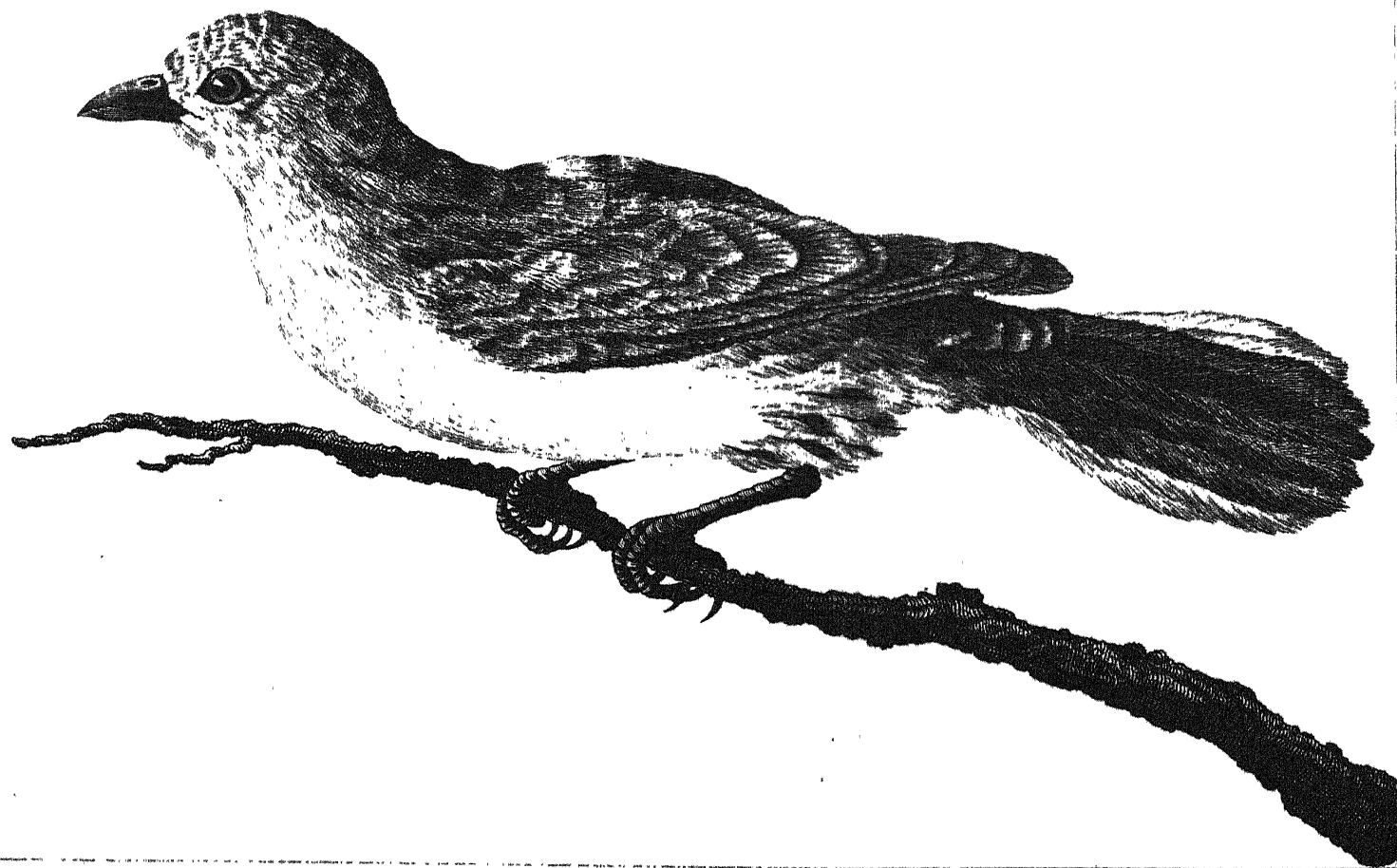
CUCULUS INDICATOR. *The Honey Guide.*

Alarum tectrices superiores omnes griseo-fuscæ, exceptis summis aliquot quæ flavis apicibus formant *maculam flavam* in humeris, exiguam, et a plumis scapularibus sæpe tectam.

> *Tectrices infra alam* albidæ, harum supremæ ex albido nigroque maculatæ.

> *Remiges omnes* supra fusci, primarii octo, secundarii sex, subtus cinereo-fusci.

> *Alulæ* griseo-fuscæ.

Cauda cuneiformis, rectricibus duodecim: harum duæ intermediæ longiores angustiores, supra et infra æruginoso-fuscæ; proximæ duæ fuliginosæ, margine interiore albicantes; duæ utrinque his proximæ, albæ, apice fuscæ, et exterius ad basin macula nigra notatæ; extima utrinque reliquis brevior, alba, apice fusca, macula nigra vix ulla ad basin.

Alæ complicatæ caudæ partem quartam attingunt.

Longitudo ab apice rostri ad extremum caudæ circiter septem uncias pedis Anglicani explet.

Rostrum a basi superiore ad apicem semunciale.

VI. *An Account of some new Electrical Experiments.* *By*
Mr. Tiberius Cavallo: *communicated by Mr.* Henley,
F. R. S.

DESCRIPTION AND USE OF THE ATMOSPHERI-
CAL ELECTROMETER.

Read Dec. 19, 1776. FIG. I. reprefents a very fimple inftru-
ment, which I have contrived for
making obfervations on the electricity of the atmofphere,
and which on feveral accounts feems to be the moft ufeful
inftrument hitherto invented for that purpofe.   A B is a
common jointed fifhing-rod, without the laft or fmalleft
joint.   From the extremity of this rod proceeds a flender
glafs tube c, covered with fealing-wax, and having a cork
D at its end, from which a pith-ball electrometer is fuf-
pended.   HGI is a piece of twine faftened to the other
extremity of the rod, and fupported at G by a fmall ftring
FG.   At the end I of the twine a pin is faftened, which,
when pufhed into the cork D, renders the electrometer E
uninfulated.

When

When I intend to obferve the electricity of the atmofphere with this inftrument, I thruft the pin I into the cork D, and holding the rod by its lower end A, project it out of a window in the upper part of the houfe, into the air, raifing the end of the rod with the electrometer fo as to make an angle of about 50° or 60° with the horizon. In this fituation I keep the inftrument for a few feconds, and then pulling the twine at H, I difengage the pin from the cork D, which operation caufes the ftring to drop in the dotted fituation L K, and leaves the electrometer infulated, and electrified with an electricity contrary to that of the atmofphere. This done, I withdraw the inftrument, and examine the quality of the electricity without any obftruction either from wind or darknefs.

With this inftrument I have made obfervations on the electricity of the atmofphere feveral times in a day, and have kept a journal of thofe experiments from the 27th of September laft to this day.

The following is the moft remarkable part of the above-mentioned journal, in which I have noted the electricity of the electrometer, that is the contrary of that in the atmofphere.

The ftroke —— fignifies *as above.*

Time of Obſervation.	Clouds.	Fog.	Wind.	Opening of the Electrometer in inches.	Electricity.
Oct. 19th, 10½ o'clock.	Cloudy.	{ Very little at a diſtance.	{ Very ſtrong.	$\frac{1}{10}$	Negative.
11	—	—	—	—	—
2	Heavy clouds.	—	Violent.	$\frac{3}{4}$	Poſitive.
2¼	Leſs cloudy.	—	Little.	1	—
3	Few at a diſtance.	—	—	$\frac{1}{2}$	Negative.
8	o	o	—	—	—
Oct. 31ſt, 11 poſt mer.	—	—	o	$\frac{1}{2}$	—
Nov. 6th, 11 poſt mer.		Very thick.	—	1	

From the above-mentioned journal I have deduced the following general obſervations.

1ſt, That there is in the atmoſphere at all times a quantity of electricity; for whenever I uſe the above deſcribed atmoſpherical electrometer it always acquires ſome electricity.

2dly, That the electricity of the atmoſphere or fogs is always of the ſame kind, namely poſitive; for the electrometer is always negative, except when it is evidently influenced by heavy clouds near the zenith.

3dly, That the ſtrongeſt electricity is obſervable in thick fogs, and the weakeſt when the weather is cloudy and there is a ſtrong appearance of rain; but it does not ſeem to be leſs at night than in the day-time.

DESCRIP-

DESCRIPTION OF THE ELECTROMETER FOR THE RAIN.

THE rain-electrometer is, in its principle, nothing more than an infulated inftrument to catch the rain, and by a pith-ball electrometer to fhow the quantity and quality of its electricity.

Fig. 2. reprefents an inftrument of this kind, which I have frequently ufed, and after feveral obfervations have found to anfwer very well. ABCI is a ftrong glafs tube about two feet and a half long, having a tin funnel DE cemented to its extremity, which funnel defends part of the tube from the rain. The outfide furface of the tube from A to B is covered with fealing-wax, as alfo that part of it which is covered by the funnel. FD is a piece of cane, round which feveral brafs wires are twifted in different directions, fo as to catch the rain eafily, and at the fame time to make no refiftance to the wind. This piece of cane is fixed into the tube, and a flender wire proceeding from the former goes through the bore of the tube, and communicates with the ftrong wire AG, which is thruft into a piece of cork faftened to the end A of the tube. The end G of the wire AG is formed into a ring, from

H 2   which

which I fufpend a more or lefs fenfible pith-ball electro-meter as occafion requires.

This inftrument is faftened to the fide of the window-frame, where it is fupported by ftrong brafs hooks at CB. The part H ftands out of the window, with the end F a little elevated above the horizon. The remaining part of the inftrument paffes, through a hole in a glafs of the fafh, into the room, and no more of it touches the fide of the window than the part BC.

When it rains, efpecially in flying fhowers, this inftru-ment, ftanding in the fituation above defcribed, is fre-quently electrified; and by the diverging of the electrome-ter the quantity and quality of the electricity of the rain may be obferved, without any danger of miftake. With this inftrument I have obferved that the rain is generally electrified negatively, and fometimes fo ftrongly, that I have been able to charge a fmall coated phial at the wire AG.

This rain-electrometer fhould be fixed in fuch a man-ner that it may be eafily taken from and replaced at the window as occafion may require; for it will be neceffary to clean it very often, particularly when a fhower of rain is approaching.

EXPERIMENTS MADE WITH A GLASS TUBE HERMETI-
CALLY SEALED, AND HAVING SOME QUICKSILVER
INCLOSED IN ITS CAVITY.

IN making ſome experiments rather foreign to elec-
tricity, it occurred to me, that when I agitated ſome
quickſilver in a glaſs tube hermetically ſealed, and in
which the air was very much rarefied, it contracted a
very ſenſible quantity of electricity; which however was
not conſtant, nor, as I firſt thought, in proportion to the
agitation of the quickſilver. Being deſirous of aſcertain-
ing the properties of this tube, I conſtructed ſeveral of
them, and, as accurately as I could, obſerved their pro-
perties; but as they all agreed with regard to the chief
points, I ſhall only deſcribe one, which is the beſt I have
yet made.

This tube is two feet and ſeven inches long, and about
four tenths of an inch in diameter: the quickſilver in
it may be about three quarters of an ounce, and to ex-
hauſt it of air, I cloſed it whilſt the quickſilver was boil-
ing in its oppoſite end.

Before I uſe this inſtrument I warm it a little and
clean it; then holding it nearly horizontally, I let the
quickſilver in it run from one end of the tube to the
other;.

other, by gently and alternately elevating and depreſſing its extremities. This operation immediately renders the outſide of the tube electrical, but with the following remarkable property, *viz.* that part or end of the tube where the quickſilver actually ſtands is poſitive, and the remaining part negative. If, by elevating this poſitive end of the tube a little, I let the quickſilver run to the oppoſite end which was negative, then the former inſtantly becomes negative and the latter poſitive. The poſitive end is always more ſtrongly electrical than the negative. If when one end of the tube (which we call A) is poſitive, that is, if, when the quickſilver is in it, I do not take off the electricity; then on elevating it ſo as to let the quickſilver run to the oppoſite end B, the end A becomes negatively electrified in a very ſmall degree: if I make it poſitive a ſecond time, and do not take off that poſitive electricity; then, on elevating it again, it appears to be poſitive in a ſmall degree: but if whilſt it is poſitive I take off that poſitive electricity, then on being elevated it appears ſtrongly negative.

Theſe appearances I would explain in the following manner: the quickſilver agitated within the cavity of the tube acts like a rubber, that is, excites the inſide ſurface of the tube poſitively, and becomes itſelf negative. Now when the quickſilver, negatively electrified, is on one

end of the tube, the outside of the glass, by the known property of charged electrics, muſt be poſitive. The remaining part of the tube being poſitive on its inner ſurface, muſt be negative on the outſide. But as there is a *vacuum* within the tube, it may be aſked, why is not the equilibrium between the negative electricity of the quick-ſilver and the poſitive electricity of the glaſs inſtantly balanced?

When about two inches of each extremity of this tube are coated with tin-foil, that coating aſſiſts to make the electricities more conſpicuous.

With regard to the conſtruction of ſuch tubes (which I have made of ſeveral lengths from nine inches to two feet ſeven inches) I find that ſome will act very well, while others will hardly acquire any electricity at all, even when they are made very hot. I am not as yet thoroughly ſatisfied with reſpect to the cauſe of this difference, but ſuſpect that the thickneſs of the glaſs is more concerned in it than any thing elſe; for I find that a tube whoſe glaſs is about one twentieth of an inch thick, anſwers better than either a thicker or a thinner one.

November 13, 1776.

VII. *A Third Essay on* Sea-anemonies.
By the Abbé Dicquemare.

ON THE GENERATION OF THE FOURTH SPECIES.

Read Jan. 9, 1777. HAVING in my second essay on sea-anemonies, communicated my disco-veries on the manner in which the fourth species is multiplied, and offered some conjectures on the internal or-ganization of the small shreds which become anemonies, I added a caution, not to admit of those conjectures with-out circumspection. What passed under my own eyes

convinced

Troisieme Mémoire pour servir à l'Histoire des Anémones de Mer.
Par M. l'Abbé Dicquemare.

Sur la Génération de la Quatrieme Espece.

Havre de Grace, Mars 7, 1776.

APrès avoir exposé dans un second mémoire pour servir à l'histoire des Ané-mones de mer, mes découvertes sur la maniere dont celles de la quatrieme espece se multiplient; après avoir ouvert quelques conjectures sur l'organisation intérieure des petits lambeaux qui deviennent anémones, je ne dissimulai pas qu'on ne devoit point y restraindre ses idées. Ce qui se passoit sous mes yeux

me

Fig. 1
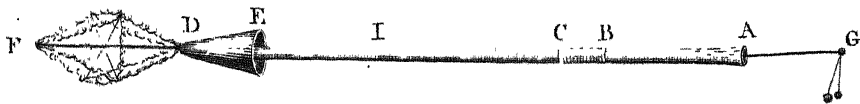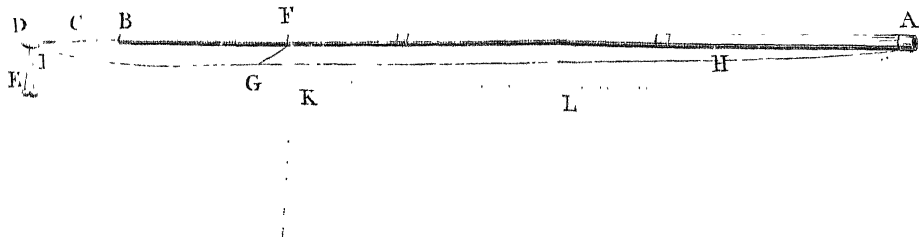
Tab.III. p.56.

convinced me more and more (and I gave fome hints to that purpofe) that the fmalleft particle of a living animal, has an organization which far exceeds every idea we can conceive of it; and which, from the extreme minutenefs of thofe particles, baffles our clofeft infpection: fo that inftead of being furprized at the fingular effects of reproduction, they are rather what we ought to expect, and be prepared to obferve as they arife. It is with this view that I have continued my experiments and obfervations: they have confirmed the difcoveries I had made, and afforded me an opportunity to juftify fome eminent men, whofe affertions concerning the multiplication of the frefh-water polypi by fections, have met with the moft unmerited contradictions.

Among all the objects which nature offers to the contemplative mind, there is none fo ftriking and important as that of the generation of beings, and efpecially of animated

me perfuadoit de plus en plus, et je m'en expliquai, que les moindres parties d'un être vivant ont une organifation qui furpaffe infiniment l'idée que nous pouvons nous en faire, que l'énorme petiteffe de ces parties dérobe aux regards les plus avides ; et que loin de fe furprendre des effets finguliers de reproduction, on devroit pour ainfi dire les attendre, et fe mettre à portée de les faifir. C'eft dans cette vüe que j'ai continué mes obfervations et mes expériences : elles ont confirmé les découvertes que j'avois faites, et me procurent la fatisfaction de juftifier les hommes illuftres, qui en nous faifant connoitre la multiplication des polypes d'eau douce par la fection, ont eprouvé les contradictions les moins méritées.

De tous les objets que la nature offre à l'efprit méditatif, il n'en eft point de plus grand, de plus refpectable, que celui de la génération des êtres, et furtout des

I

mated beings. This grand myftery has always attracted the attention of the greateft philofophers; but their want of fuccefs in their refearches may be eafily inferred from the deficiency of proper means of obfervation: and it feems an advantage referved for our age, to introduce a new fet of beings, which bids fair to throw great light upon our enquiries on that fubject.

Had I only frefh proofs to add to thofe already given, and no new difcoveries to commmunicate, I fhould certainly leave nature to eftablifh our opinion of her operations: but having been fortunate enough to gather fome fcattered fruits in this wide field, which had efcaped the obfervation of others merely becaufe they were engaged in more plentiful harvefts, I have been ftruck with the increafing fingularity, number, and importance of the objects, which have appeared to me to diffipate all doubt.

I fhall

êtres vivans: c'eft auffi vers cette merveille que les philofophes les plus diftingués ont dirigé leurs regards; mais leur manque de fuccés refulte naturellement des foibles moyens qu'ils avoient pour faire des obfervations; et c'eft un avantage qui paroit avoir été refervé à notre fiecle, de faire paroître fur la fcene des êtres jufqu'ici ignorés, qui promettent de nous fournir des nouvelles lumieres fur ce fujet.

Si je n'avois à préfenter ici que les mêmes découvertes, quoique ce fût multiplier les preuves, je laifferois à la nature le foin de fixer l'opinion; mais dans ce champ fertile j'ai été affez heureux pour recueillir quelques fruits à l'écart, et qui n'avoient échappé aux regards des autres que parcequ'ils etoient occupés de recoltes plus abondantes: j'ai vû la fingularité s'augmenter, et le nombre comme l'etendüe des objets m'a paru propre à diffiper les doutes. Je ne rappellerai pas ici

ce

I ſhall not here repeat what I have already ſaid concern-
ing the reproduction of the ſuperior part of the fourth
ſpecies, which is often as thick as one's arm; new experi-
ments have confirmed it, and have ſhewn the poſſibility
of the reproduction of freſh-water polypi, without hav-
ing recourſe to a multitude of imperceptible animals:
but the better to explain what I have ſince diſcovered
concerning the generation of this ſpecies of anemo-
nies, I muſt beg leave briefly to recapitulate what I have
already publiſhed on that ſubject. Theſe anemonies,
having their baſes unequally extended upon, and firmly
adhering at their extremities to a hard ſubſtance, contract,
and thus tear off and leave on that hard ſubſtance, one or
more ſmall ſhreds of their baſes, covered with pieces of
the coat of the old animals; and theſe ſhreds ſoon after
become ſmall anemonies, which alſo is the caſe in artificial
ſections. Of this ſingular operation I have had opportu-
nities

ce que j'ai dit de la reproduction de la partie ſupérieure des anémones de la qua-
trieme eſpece, ſouvent groſſe comme le bras; de nouvelles expériences l'ont encore
confirmée, et ont fait voir la poſſibilité de celle des polypes d'eau douce, ſans avoir
beſoin de recourir à une multitude d'animaux imperceptibles : mais qu'il me ſoit
permis de retracer ici en peu de mots ce que j'ai déja publié ſur la génération de
cette eſpece, autrement il ſeroit difficile de ſaiſir ce que j'ai apperçu depuis. Ces
anémones, ayant la baſe inégalement étendüe et fortement attachée par quelques
points de ſes extremités ſur un corps dur, ſe retirent ſur elles mêmes, et laiſſent
ainſi en ſe déchirant une ou pluſieurs portions très petites de leur baſe, recouvertes
d'une parcelle de leur robe, qui deviennent en peu de temps des petites anémones,
ce qui a lieu auſſi par des ſections violentes. En obſervant de nouveau ces

animaux

nities of feeing repeated instances, having been very affi-
duous and constant in my observations.

On the 26th of October 1775, an anemony, on which
I had tried an experiment foreign to the present purpose,
contracted, and left on the side of the vase a small shred,
which at the very first I suspected to be intended for a
young anemony: not that it was either fleshy, or that it
seemed to contain a bulb; but because the anemony had
for several days before stretched itself considerably, and
in a particular manner, from that point of the base. Cer-
tain internal fibres or *radii* appeared, which in the old
anemony had their direction from the circumference to
the centre: and as the shred was an irregular segment of
the area of the circle formed by the base of the old ane-
mony, it is evident that these fibres or *radii*, being some-
what distant from each other at the arch of the segment,

did

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animaux, et après avoir attendu longtemps, j'ai eu la satisfaction de voir se répéter
cette opération singuliere, et je l'ai suivie d'auffi près qu'il m'a été poffible.

Le 26 Octobre, 1775, une anémone fur laquelle j'avois tenté une expérience
qui n'a rien de commun avec notre objet prefent, laiffa contre les parois du vafe,
en fe retirant, un petit lambeau que je foupçonnai dés le commencement être
deftiné à devenir une petite anémone; non qu'il fut épais ou qu'il me parut contenir
quelque bulbe, mais parcaque l'anémone s'etoit fort alongée par ce point de fa
bafe, depuis plufieurs jours, d'une maniere toute particuliere. On appercevoit dans
l'intérieur certains fibres ou rayons qui dans l'anémone avoient tendu de la cir-
conférence au centre, et comme ce lambeau etoit un fegment irrégulier de l'aire
du cercle que forme la bafe d'une grande anémone, on comprend que ces fibres
ou rayons, un peu diftans l'un de l'autre à l'aré de ce fegment, ne convergoient

pas

did not converge fufficiently to form a centre at the chord; and that the point of union of thefe *radii* was the centre of a circle equal to that formed by the bafe of the old anemony (fee TAB. III. fig. 1. its natural fize, and fig. 2. magnified). During the firft days this little fragment acquired plumpnefs, bent itfelf gradually into a round figure, and feemed to make fome efforts towards forming itfelf into the fhape in which it appeared on the 25th; the fibres becoming gradually more convergent, the chord of the arch fhorter, and the arch a fegment of a fmaller circle. At length the *radii* united in a centre (fee fig. 3. magnified) and its profile appeared a fegment of a fphere (fee fig. 4.)

On the 30th of October I perceived in this fragment confiderable contractions and dilatations in the direction of its thicknefs; but could fee no mouth nor limbs. On the 1ft

of

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pas affez pour former un centre à la corde; et que le point de réunion de ces rayons etoit le centre d'un cercle égal à celui que formoit la bafe de l'anémone: voyéz TAB. III. fig. 1. et 2. dans l'une il eft de grandeur naturelle, et dans l'autre vû à la loupe. Pendant les premiers jours cette petite portion prenoit de l'epaiffeur, fe recourboit et s'arrondiffoit peu à peu; elle tendoit de toute fes forces à prendre la forme ou elle parvint le 25, c'eft à dire que petit à petit, les fibres etant devenus plus convergens, la corde de l'arc du cercle plus courte, l'arc une portion d'un plus petit cercle, il s'etoit formé un centre de réunion de ces rayons, qui occupoit le côté de ce petit corps animé, comme le reprefente la figure 3. qui eft vüe à la loupe; quant au profil, il eft réprefenté par un fegment de fphere, ou par la figure 4.

Le 30 Octobre j'apperçus des contractions et des dilatations très fenfibles dans l'epaiffeur, mais point de bouche ni de membres. Le 1 Novembre il a changé de place.

of November it moved from its place. On the 7th I difco-
vered, by means of a ftrong lens, an orifice and fome ap-
pearance of limbs. On the 16th I faw them very diftinctly.
On the 17th it changed its place again. At the beginning
of January 1776, the folds of the body were formed, and
then it could not but be confidered as a young anemony,
fimilar in every refpect to the old one by which it had
been produced, except in the number of limbs, which
however, although the animal be ftill very fmall, are
now increafing. The femi-tranfparency, which often
prevents accurate obfervation, allowed me to view the
gradation through which the fmall fibres became con-
vergent: I could alfo fee the gradual contraction of the
angles of the fegment, and that not a fingle particle of
the fragment perifhed; all feemed to be re-incorporated
into the mafs. The word *all* muft not, however, be
taken in a ftrict fenfe; for I muft confefs that I per-
ceived fome yellowifh filmy matter round this little
mafs,

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place. Le 7, à l'aide d'une forte loupe, j'ai apperçu un orifice, et des apparences de
membres. Le 16 je les ai vûs très diftinctement. Le 17 il a de nouveau changé
de lieu. Au commencement de Janvier 1776, le pli du corps s'eft formé; c'etoit
donc alors une petite anémone qui, à l'exception du nombre des membres, reffem-
bloit parfaitement à celle dont elle etoit provenüe; les membres augmentent, et
cependant cette anémone eft fort petite. La demi-tranfparence, qui gêne quel-
quefois dans les obfervations, m'a permis de diftinguer le progrès par lequel les
petites fibres font devenües convergentes: J'ai vû auffi les angles de fegment fe
raccourcir, et rien ne m'a paru périr: tout s'eft refoulé dans la maffe. Ce mot *tout*
ne doit cependant pas être pris à la lettre: j'ai apperçu quelques pellicules quelques,

maſs, which ſeparated from it; but its quantity was ſo ſmall, that I could not thence infer the loſs of any particle of the ſhred, and rather imagined that this matter was merely the effect of ſome ſecretions or extravaſated humour.

What ſtill leaves me a doubt concerning the germ is, that this effect has been more conſiderable in ſome of my experiments than in others. Had the fragment of the anemony contained a germ, it might be concluded that the membrane which covered it ſhould have periſhed as ſoon as the animal was formed; this however was not ſufficiently evident in ſeveral caſes, and eſpecially in thoſe where the fragment had been naturally torn off. At firſt the ſhred was thin, and thinner ſtill before it was ſeparated from the old anemony; and no bulb could ever be perceived either then or in the ſequel. I was then led to imagine,

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ſubſtances un peu jaunâtres, autour de la petite maſſe, qui en ont été ſeparées; mais elles etoient en ſi petite quantité, que je n'ai pu en conclure qu'il eut péri quelque choſe du lambeau, cette ſubſtance etoit peut-êtré düe à des ſécrétions ou à quelque humeur extravaſée.

Ce qui me laiſſe encore un doute réel, c'eſt que cet effet a été plus ſenſible dans quelques unes de mes expériences. Si cette portion d'anémone eût contenu un germe, n'eſt-on pas porté à penſer que les membranes qui l'envelopent auroient péri lors de ſon dévelopement. C'eſt ce qu'on ne remarque pas d'une maniere aſſéz ſenſible dans pluſieurs, et ſurtout dans ceux qui ſe déchirent naturellement. D'abord le petit lambeau étoit mince et, avant qu'il fût ſéparé de l'anémone, je l'ai obſervé pendant pluſieurs jours; il etoit plus mince encore: je n'y voyois nulle apparence

imagine, especially on account of the union of the fibres in a common centre, that a simple shred produced an anemony. But from what cause does this little fragment contract into a circle, and increase its thickness? how shall we account for its visible tendency towards forming a new animal? is it a necessary instinct? can it be admitted that in these animals the vital principle is peculiar to every particle? and in what manner is it, or is it not, subordinate to the general organization of the individual, according to circumstances which it seems in our power to modify? How many specious reasonings might be made on this subject, did we prefer the admiration of the public to real truth! The only inference that I think can as yet be derived from these and the following observations is, that there certainly are animated beings which multiply, as it were, by slips; but whether the concurrence

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apparence de bulbe, et je n'y en ai point vu depuis : j'ai donc été tenté de croire, surtout à cause de la réunion du bout des fibres à un centre commun, que c'étoit un simple lambeau qui devenoit anémone. Mais qui est ce qui fait, qu'étant détachée, cette petite portion se recourbe, et prend de l'épaisseur ? par quelle cause tend-elle visiblement à former un animal ?. sent-elle alors la necessité de le devenir ? le principe de la vie feroit-il, dans ces animaux, particulier à chacune de leurs parties ; et comment est il, ou n'est il pas, selon les circonstances que nous pouvons faire naitre, subordonné à l'organisation générale de l'individu ? Oh, si on étoit tenté de préferer l'admiration du public à la bonne foi, que de choses à dire ! Tout ce qui me semble qu'on peut conclure de ces observations, et plus encore de celles qui vont suivre, c'est qu'il est vrayment des êtres animés qui se multiplient comme de bouture ; mais j'attends être instruit par des
nouvelles

currence of two fexes may or may not be difpenfed with, is a fact I expect to learn from future experiments. I can at prefent only fufpect its being unneceffary, fince the anemonies I had hitherto operated upon were all produced at fea. The manner in which they multiply feems ftrongly to favour that fuppofition: fuch ideas begin already to become familiar to us, they appear lefs fingular; and yet how different are they from thofe we ufed to entertain of the animal creation!

Let us now proceed from the operations of mere nature, to fuch as are in fome meafure the effect of art. On the 12th of December 1773, I fevered with the point of a knife ten fmall pieces from the bafes of feveral anemonies, at the places where they feemed the moft diftended, and moft adherent to large oyfter-fhells, from which they dropped after the operation. I put thefe

pieces

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nouvelles expériences, fur l'inutilité du concours des deux fexes : je ne puis que la foupçonner, parceque les anémones que j'ai opéré jufqu'ici etoient nées en mer. La maniere dont ces animaux fe multiplient femble étayer fortement ce foupçon. Déja nous nous accoutumous à toutes ces idées nouvelles; elles nous frapent moins : cependant combien font elles éloignées de celles que nous avions de l'animal!

Paffons maintenant des opérations propres de la nature, à celles où l'art entre pour quelque chofe. Le 12 Décembre 1775, je coupai, avec le bout d'un biftouri, dix petites portions des bords de la bafe de plufieurs anémones, aux endroits ou ces bafes etoient plus etendües, et plus adhérentes à des écailles de groffes huitres, dont elles fe détachoient en les coupant. Je mis ces particules

pieces into different vases. The next day two of them stuck to the bottom; on the 14th two more adhered; on the 22d, six of them; on the 24th, nine; and on the 27th, all were affixed. Each of them went through the same progress as the pieces which had been torn off naturally; and before the beginning of March they were all furnished with limbs &c. Although I be conscious of having been very accurate and attentive in my observations, still I have not yet been fortunate enough to see all that I could wish. I shall however repeat my attempts, and I have great expectations from the new experiments I meditate. The present have pointed out some differences which lead to several reflections. Those shreds which had been cut of a larger size, produced (besides the films they may have lost) larger anemonies. Whether this multiplication be the effect of a bulb, of a germ,

<div align="right">or</div>

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dans autant de vases: le lendemain deux etoient attachées au fond; le 14 deux autres etoient de même attachées; le 22 six, le 24 neuf, et le 27 elles l'etoient toutes. Chacune d'elles m'ayant présenté successivement les mêmes progrès qu'avoient fait les portions détachées naturellement, il seroit inutile de les décrire de nouveau: avant le premier Mars elles avoient toutes des membres, &c. Quoique j'aye observé avec le plus grand soin ce qui s'est passé, je n'ai pas encore été assez heureux pour voir tout ce que je désirois; mais j'y reviendrai et j'espére beaucoup des nouvelles tentatives que je médite. Celles-ci m'ont offert des différences qui donnent lieu à quelques réflexions. Ceux de ces petits morceaux d'anémones que j'ai coupés plus grands, ont produit (indépendamment de ce qui a pu s'en détacher) des anémones plus grandes. Si c'est en vertu d'une bulbe, d'un

<div align="right">germe.</div>

or of an egg, it ftill appears that its teguments, and all that is contiguous to thofe teguments, fhould not be a confftituent part of the animal, and that only a larger germ can produce a larger animal.

Is it then in our option to produce anemonies not only when we pleafe, but alfo of what fize we pleafe[a]? Or does the multiplying anemony follow in this its own inclination? All this, added to the junction of the fibres, feems very oppofite to the opinion of the germs or eggs; but on the other hand there are obfervations which fa-

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[a] Without invalidating what is here faid, fome experiments have induced me to think that this affertion fhould be admitted with fome limitations; that if the fhreds be very large, they will perifh; that in general only fmall ones fhould be cut, without either fretting or tearing them; and that the vafes fhould always be kept very clean, and the water as clear and as frefh as poffible.

your

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germe, ou d'un oeuf quelconque, que cette multiplication fe fait, il femble que tout ce qui y feroit joint comme envelope, et plus encore comme contigu aux envelopes, devroit ne pas faire partie du petit animal, et qu'il n'y auroit qu'un germe plus gros, qui donnât une anémone plus groffe.

Sommes nous donc les maitres, non feulement de faire naitre les anémones quand nous voulons, mais même de leur donner plus ou moins de groffeur [a]? L'anémone qui multiplie en difpofe-t-elle auffi à fon gré? Tout cela joint à la réunion des fibres, femble bien oppofé aux germes et aux œufs; mais auffi d'autres obfervations leur font favorables. Il m'a femblé qu'il fe féparoit un peu plus de

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[a] Sans affoiblir ce que j'éxpofe, quelques expériences me font penfer que ceci a des bornes affez étroites; que des morceaux trop grands périffent, et qu'en général il n'en faut couper que de petits, fans les tirailler; que les vafes doivent être nets, et l'eau claire et fraiche.

vour it. It appeared to me, that more of the membrana-
ceous particles came off from the shreds which had been
clipped of a larger size, than from those which had been
naturally torn from the anemony; but as I have not had
occasion to observe many of the latter, this difference
may be owing merely to the different sizes of the shreds.

Another circumstance I observed in the pieces that
were naturally torn off is, that there are some which
produce several anemonies, which sometimes remain
united, and at other times separate. I have myself fre-
quently seen this operation. One of the shreds I had clip-
ped was of an irregular shape, nearly as in fig. 5. A little
contraction was soon formed between the two extremities,
both of which became round, swelled and assumed the ap-
pearance of two drops of tallow; the contraction became

like

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ces especes de pellicules des morceaux coupés un peu gros, que de ceux arrachés
naturellement par l'anémone même ; mais comme je n'ai eu occasion d'observer
qu'un petit nombre de ceux-ci, il pourroit arriver que cette différence ne seroît
dûe qu'au plus ou moins d'étendüe du lambeau.

Une autre remarque que j'avois déja faite dans les morceaux séparés natu-
rellement est, qu'il s'en trouve quelques uns d'où naissent plusieurs anémones,
entre les quelles il y en a qui restent unies, tandis que d'autres se séparent. Cette
opération s'est répétée sous mes yeux. Un lambeau que j'avois coupé avoit à peu
près la forme que représente la figure 5. Il se forma un petit étranglement
entre l'un et l'autre de ses bouts ; chaque bout prit une figure ronde, et leur sur-
face supérieure s'elevoit en goute de suif : l'étranglement devint comme un
filet,

like a thread (fig. 6.). On the 24th of January, the largeft piece (for they were of very unequal fizes) crept up a little way on the fide of the vafe; and on the 28th, the thread broke, when the two fragments became two diftinct anemonies. Doth this imply that there were two germs in the fhred? Or may one fingle fhred, without any bulb, germ, or egg whatever, produce one or more anemonies either connected or feparate? Thefe are queftions I am not yet able to folve, and I wifh I were the only one thus uninformed. But to return; the following facts, of which I have frequently been eye-witnefs, muft now be admitted. 1ft, That the anemonies of this fpecies are multiplied by fhreds, both naturally and artificially. 2dly, That thefe fhreds produce fometimes only one, and at other times feveral anemonies. 3dly, That among thefe young anemonies formed by one fhred, feveral re-
main.

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filet, figure 6. Le 24 Janvier, la plus groffe partie (car elles etoient fort inégales) monta un peu aux parois du vafe; le 28 le filet s'eft rompu, et ces deux parties font devenües deux petites anémones. Y auroit il donc eu deux germes dans ce morceau? ou bien peut-il d'un fimple lambeau fans bulbe, fans germe, fans œuf quelconque, naitre une ou plufieurs anémones, unies ou féparées? C'eft ce que je ne fçais pas encore, et je voudrois être le feul qui l'ignorat. Revenons donc à cette fource féconde. D'abord on doit regarder comme des faits certains, dont j'ai été temoin plufieurs fois, 1°, que les anémones de cette efpece fe multiplient naturellement et artificiellement par des lambeaux; 2°, que ces lambeaux deviennent fouvent une, quelquefois plufieurs anémones; 3°, que de ces petites anémones, formées d'un même lambeau, plufieurs reftent unies entre elles.
quoique.

main connected, but that the greatest number separate by contractions. 4thly, That among those which remain connected there are some which grow to the largest size, such as the monstrous anemony mentioned in my second essay, in which three individuals had been blended together; and another of a smaller size in the shape of a Y [b], represented in the plate of the same essay, which before my own eyes produced a young anemony, by tearing a small shred from the base of its coat.

What do we then perceive in these shreds? Nothing hitherto but a membrane which was before part of the base of the great anemony, a mere skin which was part of

(b) This species, which is so fertile in monstruosities, has also presented me with one which had two bodies on one base. Of the first species I have as yet seen but one monster, which on the contrary had two bases and only one body. I saw it at its birth.

its

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quoique le plus grand nombre se sépare par étranglement; 4°, que parmi celles qui restent unies, il y en a qui deviennent de la plus belle grosseur, comme l'anémone monstrueuse dout j'ai parlé dans mon second mémoire, dans laquelle trois individus etoient confondus, et une autre moins grosse en forme d'Y (b), repre-sentée dans la planche du même mémoire, qui produisit devant moi une petite anémone en déchirant un lambeau du bord de sa base.

Qu'apperçoit-on donc dans l'un de ces lambeaux? Jusqu'ici je n'y ai vû qu'une membrane qui faisoit auparavant partie de la base de la grande anémone, une

(b) Cette espece féconde en monstruosité m'en a encore offert une qui a deux corps sur une même base. Je n'ai jamais eu qu'un monstre de la prémiere espece, il avoit au contraire deux bases et un seul corps. Je l'avois vû naître.

its coat; fome mufcular fibres and fmall internal fila-
ments defcribed in my fecond effay, as they appeared in
the folar microfcope; and a clammy fubftance filling up
the interftices. When fuch a fhred is decompofed, it
changes into a whitifh glutinous fubftance, which,
through a microfcope, appears a mafs of minute globu-
lar bodies, that feem ftill to be of a compound texture,
and fome of which are of a larger fize, and of an oblong
oval fhape (fig. 7.) as they may be often obferved in
fea-water viewed through a microfcope. The circular
edge of thefe fhreds which formed part of that of the
old anemony, retains the faculty of adhering and loofen-
ing itfelf; nor do any of the fibres perifh in the forma-
tion of the new animal; they only receive a new arrange-
ment, or acquire a greater convergency. What caufe
can

peau portion de fa robe, des fibres mufculaires, des petits cordons dans l'inté-
rieur dont j'ai parlé dans mon fecond mémoire aprés les avoir examinés au micró-
fcope folaire, et enfin une matiere gélatineufe qui remplit les intervalles.. Lorfque
ces lambeaux fe décompofent, il en refulte une matiere blanchâtre, legérement
vifqueufe, l:quelle, vûe au microfcope, offre des efpeces de corps globuleux, très
petits, qui paroiffent compofés, et parmi lefquels on renarque une grande quan-
tité d'animaux plus grands, d'un ovale fort allongé, comme le reprefente la figure
7, et comme on en voit fouvent dans l'eau de la mer éxaminée au microfcope.
Le bord en portion de cercle de ces lambeaux, qui formoit celui de la grande ané-
mone, conferve la faculté de s'attacher et de fe détacher; et dans la formation du
petit animal les fibres ne fe détruifent point, ils ne font que prendre comme l'on
voit un nouvel arrangement, ou plus de convergence. Quelle peut donc être
la

can we then affign to the tendency thefe fibres and thefe
fhreds have to form a new animal? It can eafily be con-
ceived that the exterior border of the fhred, preferving
the faculty of adhering, may form a fegment of a fmaller
circle; but it might equally incline to form a fegment of
a larger one, did it not feem more natural that a body en-
dowed with fenfation, fhould rather endeavour to clofe a
wound, than to open and rend it more and more. There
muft then be in this fhred a certain degree of fenfation,
fince in order to fix, and to loofen itfelf, occafionally, it
muft have a perception of its adherent or detached ftate.
But there is even more than this in the fhred we are ex-
amining; for allowing that the border affumes a circular
fhape, and thereby caufes a convergency of the fibres,
that the angles contract, and the wound clofes, the refult
of which is a fmall animated body; yet it will always be

difficult

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la caufe par laquelle ces fibres, et le lambeau entier, tendent à former, et femblent
former en effet une petite anémone? On conçoit aifément que le bord extérieur
du lambeau, confervant la faculté de s'attacher, peut former une portion d'un plus
petit cercle; mais auffi il pourroit en former une d'un grand, fi ce n'eft qu'il paroit
plus naturel pour un corps doüé de la faculté de fentir, de tendre à refermer
une playe, que de l'ouvrir, ou de la tirailler de plus en plus. Il y a donc dans ce
petit lambeau une forte de fenfation, puifque pour s'attacher et fe détacher au
befoin, il femble qu'on doit fentir qu'on s'attache ou qu'on fe détache. Il y a
plus que cela dans ce lambeau que nous obfervons, car quand le bord s'arrondi-
roit, que par là les fibres prendroient de la convergence, que les angles fe rap-
procheroient, que la playe fe confolideroit, qu'en réfulteroit-il? un petit corps
animé?

difficult to conceive how this fmall fhred preferves all thefe faculties, how this animated being acquires the power of loco-motion, and whence proceeds the whole reproduction of an animal. Where fhall we find the principles of the limbs, of the inteftines, of the mouth, of the fold in the body, of the elegant tufts which terminate the limbs, &c. fince nothing of all this can be obferved in fuch a fragment? Shall we feek for that principle in the flender threads, the conftruction of which, feen with the folar microfcope, had ftruck me with admiration? but my obfervations have not convinced me that they were intended for thefe purpofes. What would have determined my opinion in favour of thefe filaments is, the facility with which they wrap themfelves up in a fpiral, and form certain parcels (which have the appearance

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animé? mais outre qu'il fera toujours difficile de concevoir comment ce petit lambeau conferve ces facultés, et comment ce corps animé acquiert celle de changer de lieu au befoin, on peut encore demander d'où procédera le dévelopement qui fuit? où fe trouvera le principe des membres, des inteftins, de la bouche, du pli du corps? d'où naiffent ces beaux panaches qui terminent les membres, &c. puifque rien de tout cela ne fe remarque dans cette partie? Chercherons nous ce principe dans les cordons déliés dont la ftructure, au microfcope folaire, nous a paru fi admirable? mais je n'ai pu trouver dans l'obfervation de quoi me convaincre qu'ils foient deftinés à cet ufage. Ce qui eût été bien plus capable de me fixer fur ces cordons, c'eft la facilité qu'ils ont de fe rouler en fpirale, et de former des efpeces de paquets qu'on apperçoit comme un

pearance of fmall whitifh bodies) near the border, be-
tween the fkin and the bafe of the young anemonies,
when they extend the latter. I might have been parti-
cularly influenced by the number of floating tranfparent
globules, which, by means of the microfcope, I have
often feen among thofe filaments, and which appeared
nearly of the fame texture: but it muft be here obferved
that the illufions of the microfcope are often very great,
on account of the fpherical form and tranfparency of
thofe globules, which at firft fight appear to have a large
hole diametrically through them. The globules which,
by means of the microfcope, are often feen in fea-water,
evidently contain a liquid; and here all my knowledge
ends. Does the principle we are in fearch of exift in
this gelatinous fubftance, concerning whofe texture we

<div align="right">are</div>

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petit corps blanchâtre vers les bords, entre la peau et la bafe des jeunes anémones,
lors qu'elles l'étendent. J'aurois pu encore être arrêté d'une maniere plus par-
ticuliere à l'afpeȼt de certains globules flotans et tranfparens, que j'ai fouvent
trouvés au microfcope parmi ces cordons, et qui me paroiffent compofés à peu
prés de même, autant que les illufions microfcopiques permettent d'en juger; car
ces illufions font fort grandes à caufe de la fphericité et de la tranfparence de ces
globules, qui au premier coup d'œil femblent percés diamétralement d'un gros
trou; et le microfcope folaire ne m'a pas mieux fervi que les autres à ce fujet. Ces
globules, qui d'ailleurs fe rencontrent fouvent dans l'eau de la mer vüe au mi-
crofcope, font fans doute remplis d'une liqueur; et voila où fe terminent toutes
mes connoiffances. Sera-ce dans la fubftance gélatineufe, dont la texture ne nous

<div align="right">eft</div>

are as yet perfectly in the dark? If ever we difcover this texture, it will probably lead us back to a more diftant mechanifm, and this to another of a ftill more delicate order: this laft perceptible caufe, will probably conduct us to the general order, that is to nature; and nature to its Author. But how fatisfactory, how ufeful may it not prove to unravel by degrees (although perhaps with fome uncertainty) even the coarfeft mechanifm by which nature operates? Shall we fuppofe that the ge-latinous matter is nothing but an irregular, inco-herent fubftance? At firft fight the fame might be faid of the white fubftance of the brain, although it feem to have more confiftency; yet in many places it ap-pears fibrous, and if we could trace it through the nerves, we fhould no doubt difcover a moft admirable or-ganization. The operations of nature in eggs, chryfa-

lids,

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eft point connüe, qu'exiftera le principe que nous cherchons? Si nous la con-noiffons un jour, elle pourra bien nous renvoyer à un méchanifme plus eloigné; celui-ci à un d'un ordre plus délicat; et enfin la derniere caufe apperçue, à l'ordre général, c'eft à dire à la nature; et la nature à fon auteur. Mais combien n'eft il pas fatisfaifant, combien ne peut il pas être utile, de déveloper peu à peu, quoiqu'avec quelque incertitude, le méchanifme même le plus groffier par lequel la nature opére? Cette matiere gélatineufe ne feroit elle qu'un corps informe fans texture? à la premiere vûe nous en dirions bien autant, de la fubftance blanche du cerveau, quoique plus compacte; cependant en plufieurs endroits elle paroit fibreufe, et fi nous pouvions y fuivre les nerfs, nous découvririons fans doute une organifation admirable. Ce qui fe paffe

dans

lids, nymphs, and a great number of marine bodies, seem to justify the opinion that there is, in certain humours, a kind of organization which is imperceptible to our eyes, and conceals from us great and important mysteries. All this must then be supposed to exist in the gelatinous matter contained in our small fragment: and it is this, perhaps better than the reproduction of parts, that will point out to us at large, the imperceptible operations of nature in the formation of the *fœtus*, of eggs, and perhaps of all that may be called a germ. It is thus that the different views taken of nature may mutually clear up each other. I have not the vain presumption to think that I shall be able to make a great progress in this dark career; all I boast is, that I have dared to enter into it. I have great expectations from the experiments I

<div align="right">have</div>

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dans les œufs, dans les chryfalides, dans les nimphes et dans une afféz grande quantité de fubftances qu'on trouve dans la mer, &c. porte à croire qu'il y a dans certaines humeurs une forte d'organifation qui nous échappe et qui couvre de grandes merveilles. Il faudra donc fuppofer tout cela dans la matiere gélatineufe de notre petit lambeau : là, plus encore que dans les reproductions de parties, elle pourroit nous offrir en grand, ce qui précede en très petit la formation du fœtus dans l'œuf, et peut-être celle de tout ce qu'on peut appeller germe. C'eft ainfi que les différens points de vüe fous lefquels on peut confiderer les opérations de la nature, pourroient s'eclairer mutuellement. Je n'ai pas la folle préfomption de croire que je ferai afféz heureux pour faire beaucoup de chemin dans cette carriere obfcure, je me fens feulement le courage d'y entrer.

have in view; but I fhall be obliged to take up the fub-
ject far backwards. An accident has juft now deprived
me of my twelve little anemonies: the fea-water was, in
in the firft days of March, fo much troubled by the fall-
ing in of part of the cliff, &c. that my great anemonies
of the firft and third fpecies have confiderably fuffered,
and all the fmall ones of the fourth fpecies died in one
day. Another accident had juft preceded this, and had
occafioned the lofs of a whole year's labour: on the 28th of
January, REAUMUR's thermometer fell to 15° below the
freezing point; I had then forty vafes containing anemo-
nies on which I was making experiments, and was at the
fame time attending to other avocations: notwithftand-
ing all the meafures I had taken to prevent it, the water
froze in feveral of them: my anemonies however would
not have died, fince one of the firft fpecies, which I had

on

Je compte beaucoup fur les expériences que je médite, mais je ferai obligé de
reprendre les chofes de loin. Un accident vient de me priver de mes douze petites
anémones : l'eau de la mer, dans les prémiers jours de Mars, a été tellement trou-
blée par l'eboulement des falaifes, &e. que mes grandes anémones de la prémiere
et de la troifieme efpece ont confidérablement fouffert, et les petites de la
quatrieme font toutes mortes en un même jour. Un autre accident avoit précédé,
et m'avoit fait perdre un an de travail: le 28 Janvier, le thermometre de
REAUMUR defcendit à 15° de condenfation; j'avois alors quarante vafes d'ané-
mones de mer en expérience, et mes foins étoient partagés par d'autres objets :
quelque précaution que j'aye pris, l'eau de plufieurs gela; mes anémones n'en

feroient

on purpofe fuffered to freeze out of the water, did not perifh; but they fuffered a good deal, and I am determined to avoid all uncertainty in my experiments.

If we judge of the multiplication of the anemonies of the fourth fpecies, by the number of young ones that are feen round the large ones, it muft be very confiderable. This fpecies alfo affords us a fingularity which is not obfervable in the frefh-water polypi, that of multiplying by tearing off, of its own accord, fmall fhreds from its body.

Although the main object of this effay be the generation of the fourth fpecies, I cannot however omit obferving that feveral of its individuals, having been cut into two equal parts perpendicularly to their bafes, formed each of them two compleat anemonies. And I beg leave to infert here an obfervation on the firft fpecies, which

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feroient pas mortes, puifqu'une de la prémiere efpece, que j'ai fait geler exprès à fec, n'a point péri : mais elles ont beaucoup fouffert, et je ne veux aucun equivoque dans mes expériences.

A en juger par le nombre des petits qu'on trouve autour des groffes anémones de la quatrieme efpece, leur multiplication eft très nombreufe : elle offre auffi, comme on vient de voir, une fingularité que ne nous ont point prefenté les polipes d'eau douce, celles de fe multiplier en s'arrachant elles mémes des petits lambeaux.

Quoique ce mémoire ait pour objet la génération de la quatrieme efpece, je ne laifferai pas d'indiquer que plufieurs de fes individus, coupés en deux portions égales perpendiculairement à leur bafe, ont formé chacun deux anémones : et d'inferer

which will in fome meafure fupply the want of an effay, the publication of which is retarded by the abovemen-tioned accident.

A FURTHER OBSERVATION ON THE FIRST SPECIES.

TOWARDS the end of the year 1774, I cut in two, in a perpendicular direction, an anemony of the firft fpecies, which had been formed from a moiety of one I had cut before, fo that each half was then only a quarter of the primitive anemony. Thefe two halves had the fame fate as the firft fections; and one of them, after having been thus reftored, and having been always kept by itfelf, produced, on the 1ft of June 1775, a young ane-mony as perfect as thofe that are produced at fea, and of the fame colour as the mother. It muft be remembered that

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d'inferer ici une notice fur la prémiere efpece qùi fuppléera en partie au mémoire dout les accidents que je viens de rappoter retarderont la publication.

NOTICE SUR LA PRÉMIERE ESPECE.

A LA fin de 1774, je coupai par la moitié, dans une direction perpendicu-laire à fa bafe, une anémone de la prémiere efpece, qui après l'avoir été, s'etoit reformée : chaque moitié n'etoit donc alors que le quart de l'anémone primitive. Ces deux parties ont fait comme la prémiere fois ; et une d'elles, après s'être ainfi reformée, et avoir toujours vécu en particulier, me donna, le prémier Juin 1775, une petite anémone, auffi parfaite que celles qui naiffent à la mer, et de la même couleur que la mere. Qu'on j'uge de ma furprife quoique je m'attende à tout ; et

that in this species, the young ones are entirely formed in the inside of the animal, whence they are put forth through the mouth; so that, whatever idea we may form to ourselves of this species of beings, we can find no real analogy between any sections from them, and those torn from stems and roots of certain trees, with a view to multiply them. The young anemony we are speaking of is not large enough to favour the supposition that it had been ready for birth, in that part of the old anemony, for more than two years before; since some young anemonies of this species, which I had kept in order to observe their encrease, have in ten months time grown to double the diameter of their bases, without my having ever taken the trouble to feed them: and it is besides known, that it is usual for these anemonies, when they

are

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si on me demande, comment ceci, comment cela? je répondrai sans rougir (puisqu'on peut le faire maintenant) je n'en sçais rien.   On se ressouvient sans doute que, dans cette espece, les petits sont entiérement formés au plus intérieur de l'animal, d'ou ils naissent plus ou moins gros par la bouche : ainsi on ne pourroit, quelqu' idée qu'on se fit de ces animaux, trouver d'analogie réelle entre ces sections, et celles qu'on fait aux tiges et aux racines de certaines plantes pour les multiplier.   La petite anémone dont nous parlons n'est pas assez grosse pour qu'on puisse penser qu'elle ait resté prête à naitre depuis plus de deux ans dans cette partie de l'anémone primitive, puisque des petits de cette espece, que j'avois précédemment gardés pour en observer l'acroissement, ont augmenté du double de leur diamètre en dix mois, sans que je prisse la peine de les nourir ; et on sçait d'ail-

leurs

are put to any pain, to eject all the young ones they contain.

The reftored moiety produced another young ane-mony on the 7th of Auguft, another on the 27th, one more on the 1ft of September, a larger one on the 20th of October; whereas to this day the other half has not yet afforded me a fingle young one. Another circum-ftance worth notice is, that the two halves of the origi-nal anemony never produced any young ones, neither during their recovery, nor after their reftoration.

Some perfons, who intereft themfelves in the progrefs of my experiments, induced me on the 27th of June laft, to cut an anemony of the firft fpecies perpendicu-larly into four parts. For this purpofe I chofe a very large one, juft taken out of the fea; and on cutting it in that manner, it put forth twelve young ones. One of thefe

leurs qu'il eft affez ordinaire qu'une anémone, lorfqu'elle fouffre quelque incom-modité notable, pouffe dehors les petits qu'elle renferme.

Revenons à notre moitié réformée : elle m'a donné une autre petite anémone le 5 Août, une le 27, une le 1 Septembre, enfin une plus grande que les autres le 20 Octobre : jufqu'au 7 Mars 1776, que j'écris ceci, l'autre partie ne m'a point encore donné de petits. Une chofe qui eft à remarquer, c'eft que les deux moitiés de l'anémone primitive, ni en fe réformant, ni après s'être reformées, ne m'en avoient donné aucun.

Quelques perfonnes qui s'intéreffent à mes expériences, m'inviterent le 27 Juin dernier, à couper, perpendiculairement à fa bafe, une anémone de la prémiere efpece, en quatre parties, comme j'en avois coupé en deux : j'en pris une affez

thefe quarters adhered the next day to the fide of the vafe: on the 30th it had crept to the furface of the water. It looked healthy till the middle of November; but at the end of the year the wound was not yet perfectly healed: neverthelefs, a few days after, it put forth a young one of a tolerable fize. The cold of the 28th of January feems to have accelerated its diffolution. The fecond quarter had nearly the fame fate, except that it yielded no young ones. The third produced, on the 6th of September, a young one of a very fmall fize. On the 15th the wound feemed to be clofed, but its place ftill appeared of a pale colour, tranfparent, and confiderably depreffed. On the 30th it put forth another fmall young one; and on the 26th of October, a third of a fomewhat larger fize. On the 29th of January 1776, after the water had been often frozen, I faw it put forth three young

groffe qui venoit d'être péchée, et en la coupant ainfi, elle me donna douze petits. Une de ces portions s'attacha le lendemain aux parois du vafe : le 20 elle etoit montée à fleur d'eau : elle a fait bonne figure jufqu'à la moitié de Novembre : à la fin de l'année la playe n'etoit qu'imparfaitement confolidée ; peu de jours avant, fans avoir l'air vigoreux, elle avoit donné un petit un peu fort. Le froid du 28 Janvier a paru accélerer fa ruine. Il en a été à peu près de même de la feconde portion comme de la précédente ; mais elle n'a point donné de petits. La troifieme m'a donné un petit d'une petiteffe extréme le 6 Septembre. Le 15, la playe paroiffoit confolidée ; mais la marque en etoit très fenfible par la couleur pâle, la tranfparence, et un refte d'applatiffement. Le 30 elle m'a donné un petit très petit, et le 26 Octobre un un peu plus gros. Le 29 Janvier

young ones of a moderate fize. On the 31ft it produced
a feventh; but fince the froft it has ever appeared in a
weakly ftate. The fourth quarter, after feveral changes
in its ftate of health, produced, on the 29th of Novem-
ber, three young ones; one large, the other of a mid-
dling fize, and the third very fmall. After the fevere
cold it declined: it neverthelefs put forth three more
young ones, and died one of the firft days in March.
Thus from three of thefe quarters I had no lefs than
fourteen young ones, befides the twelve the animal had
produced during the operation. The interior contrac-
tions of the anemonies not only renders it difficult to cut
them into four parts; but I have alfo obferved, that thofe
fections do not eafily recover a cylindrical form, and
that they are eafily affected by froft or any other acci-
dent. This therefore is an experiment of little ufe,

which

1776, après que l'eau fut glacée plufieurs fois, j'ai vû naitre trois petits de
moyenne groffeur. Le 31 Fevrier elle m'en a donné un feptieme; mais depuis la
gelée, elle a fait mauvaife figure. Après des alternatives de bon et de mauvais
état, la quatrieme s'eft réformée, et m'a donné le 29 Novembre trois petits, l'un gros,
l'autre moyen, et le troifieme très petit. Depuis le grand froid elle alloit mal; elle
a cependant donné encore trois petits, et s'eft décomp' fée dans les premiers jours
de Mars. Voici donc quatorze petits donnés par trois de ces portions, indépen-
damment des douze que l'anémone avoit rendûs en la coupant. Non feulement il eft
difficile de couper exactement une anémone en quatre, à caufe des contractions
intérieures; mais même j'ai remarqué que ces portions avoient bien de la peine
à reprendre une forme cylindrique, et que les accidens comme la gelée et autres,

les

which I only relate on account of the number of young ones; and to shew, that even a quarter of an anemony tends towards resuming, and does sometimes actually resume the cylindrical form, which is the figure of the whole animal when it dilates itself.

les affectent beaucoup. C'est donc une expérience peu fructueuse, que je ne rapporte qu'à cause des petits ; et pour faire connoitre que jusqu'à un quart d'anémone tend, et peut même parvenir quelquefois à reprendre la forme cylindrique qu'à l'anémone entiere lors qu'elle se dilate.

VIII. *Experiments and Observations in Electricity.*
By Mr. William Henly, *F. R. S.*

P A R T   I.

*Remarks on the effects of lamp-black and tar, or lamp-black
and oil, as protectors of bodies from the stroke of light-
ning; with similar effects produced by experiments in the
artificial electricity.*

Read, Jan. 16 and 23, 1777.

DR. LEWIS, in his Philosophical Com-
merce of Arts, p. 364. mentions a
remarkable instance of the effect of a coating of lamp-
black and tar, in preserving those parts of the mast of a
ship, which were covered with it, from damage by a
stroke of lightning which shivered the other, that is, the
uncoated parts of it, in a very extraordinary manner.
The account is recited at large in vol. XLVIII. of the
Philosophical Transactions. Captain NAIRNE, in his
Remarks on the Effects of Lightning, on the masts of
several vessels which were struck in the Bason at Que-
bec[a], mentions, in his letters to some friends in London,
no less than five instances, where the lightning passing

(a) His own ship, the *Generous Friends*, was twice preserved there by
his conductor.

over thofe parts of the mafts of the fhips which were covered with lamp-black and tar, or painted with lamp-black and oil, without the leaft injury, fhivered the un-coated parts (tearing out fplinters five or fix feet in length, and fix or eight inches deep) in fuch a manner as to render the mafts entirely ufelefs. A very curious inftance of this kind hath lately been communicated to me by a learned and ingenious member of the Royal Society, from whom I received the account which I fhall here infert *verbatim*,

   " On the firft of April, 1776, being on board a brig
" in the latitude 34° N. our veffel was ftruck with light-
" ning, which difabled our fore-maft. Upon getting the
" whole of the top-mafts down upon the deck, I ob-
" ferved the following particulars, with regard to the
" courfe and effects of the electrical matter. The light-
" ning had firft ftruck the pinnacle of the fore-top-gal-
" lant-maft (on which, at that time, there happened to
" be no iron fpindle, as ufual) which it juft fplit flightly
" for about two feet and a half, where it was painted
" with lamp-black and oil (I could not, however, afcer-
" tain whether this part of the maft had been newly
" fplit or not: it might have been firft occafioned by
" driving in the iron fpindle, whereon a vane is ufually
" placed; for there was a hole where fuch a fpindle had
                       " been

" been fixed) and then the lightning immediately fhi-
" vered the reft of that maft as far as it was greafed, till
" it met the bottom of that and the top of the top-maft,
" which had likewife been coated over with lamp-black
" and oil, on an old coat of tar. Over thefe it glanced,
" without any perceptible effect, till it reached that part
" of the top-maft which was alfo greafed; this part it fhi-
" vered, fplitting off large fplinters which were thrown
" on the deck. The lower part of the fore-maft being
" tarred, the lightning flid over it for about nine
" feet, without the leaft mark of violence; then darted
" into the fore-maft, where it was greafed; rived it ter-
" ribly, tearing off fplinters as large as a man's arm, and
" four or five feet long, all the way down. It continued
" this courfe till it again met a coat of tar, which was
" laid on the maft for more than five feet above the
" deck; here its effects on the maft difappeared, and its
" courfe feemed to be divided different ways by two at-
" tractive bodies of iron; one, in a cleet faftened to the
" fore-maft, about ten inches below the top of the laft
" mentioned coat of tar, by two large fpike-nails: this
" cleet with the nails was entirely ftruck off; the light-
" ning then tore the canvas coating round the foot of
" the maft (about a yard below the cleet) without any
" other hurt to the maft itfelf; and from thence was
" attracted:

" attracted by a large anchor lying in a horizontal poſi-
" tion on the deck, about two feet from the maſt. All this
" part of the electrical matter ſeems to have been then
" diſcharged by the different conductors of the two flukes
" and the main ſhank of the anchor, without any other
" viſible effect than breaking a piece out of a large iron
" pot, ſtanding in an immediate direction to one of the
" flukes, and about ten inches diſtant. The other divi-
" ſion of the electrical matter darted from the maſt to
" the belfry, about fifteen inches diſtant, tore off two
" cleets faſtened to it by large iron nails; then deſcended
" to the large iron hinges which hold the palls or ſtops
" (joined to the belfry-poſts) of the windlaſs, where,
" after tearing off a ſmall nail or two, it totally diſap-
" peared. The belfry which was painted was much
" ſplit, but not broke to pieces. The moſt remarkable
" part of this accident, and for which the whole relation
" is given, is, the effect of the tar and lamp-black and
" oil as repellents of the electrical matter; for in four
" inſtances, 1ſt, at the head of the fore-top-gallant-maſt;
" 2dly, at the bottom of that and head of the top-maſt;
" 3dly, at the bottom of the top-maſt and head of the
" fore-maſt; and 4thly, at the bottom of the fore-maſt;
" the lightning had glanced over all thoſe parts without
" doing the leaſt damage, and had regularly darted into,

<div align="right">" and</div>

" and ſhivered all the intermediate parts of thoſe maſts,
" where they were covered with greaſe. This appears
" ſo extraordinary to me, that I thought proper to take a
" note of it upon the ſpot, in order to occaſion the making
" of ſome philoſophical inquiries and experiments on
" the ſubject by thoſe more capable of accounting for it
" than myſelf: and for that reaſon the above relation
" may be depended on as juſt and true, after a repeated
" and critical examination of the ſeveral parts of the
" maſts which I have deſcribed.   W. D."

In conſequence of this relation, and the hint which concludes it, I have been induced to make the following experiments. 1ſt, A glaſs tube, eight inches long, with a bore or cavity of about a quarter of an inch diameter, being filled pretty cloſely with lamp-black, and each end ſtopped with a bullet, the ſtem of which juſt entered, and was cemented in the cavity of the tube, conducted the charge of a jar containing three ſquare feet of coated ſurface inſtantaneouſly, but with ſcarce any exploſion. 2dly, Such a tube being filled with a mixture of lamp-black and oil (as uſed by the painters) entirely failed to conduct the ſhock. 3dly, The outer ſurface of ſuch a tube being painted with lamp-black and oil, and excited with dry warm flannel, acted (the tube being alſo dry and warm) as a very ſtrong negative electric. 4thly, A piece

of polifhed plate glafs being introduced into the circuit, the ends of the wires which compofed the circuit were laid at about an inch and an half from each other upon the furface of the glafs, when the jar above-mentioned being difcharged, the polifh of the glafs was always torn off in an irregular and deep line, extending from one wire to the other; but if a piece of glafs painted with lamp-black and oil were thus introduced into the circuit, and the difcharge made as before, not the fmalleft trace of the electricity could ever be perceived on its furface. 5thly, If inftead of the glafs, a flip of writing-paper was introduced into the circuit, it was torn in pieces by the explofion, much of it flying about the room in the form of fine flue or down. 6thly, A flip of the fame paper, painted with lamp-black and oil, received not the leaft injury, nor fhewed the fmalleft trace of the electricity upon its furface; but the common oiled paper, without lamp-black, was torn confiderably, though not fo much as the paper unoiled[b]. When the charge of the jar above-mentioned is made to pafs between the furface

(b) Mr. CAVALLO, who hath fince repeated thefe experiments, finds, that if the paper be very thinly painted with the lamp-black and oil, it will be torn by the explofion; but having tried a piece of the fame that I had ufed in my experiments, he was not able to make the leaft impreffion on it. Lamp-black and tar therefore, on account of the greater tenacity of the latter, and its being equally a non-conductor, feems to be the moft proper for the purpofe.

of thick plate glass, and that of a cylinder of ivory three-quarters of an inch diameter, pressed by a weight of about six or eight ounces Troy; the glass is always shivered into very small fragments, and part of it is sometimes reduced to an impalpable powder. But 7thly, If the plate of glass be covered with a slip of writing-paper, painted with lamp-black and oil, or with a slip of oiled silk (such as is frequently used for garments) the charge passes over these substances without leaving the smallest trace on either of them, though the glass under them be broken by the blow of the explosion.

From these experiments, and the observations above recited, I think the following corollaries may be deduced. 1st, That a charge of electricity, or a stroke of lightning, which is the same thing, passes, in many cases, upon the surface of bodies, in a much larger proportion than through the interior substance of them, as appears by the masts of ships, coated with lamp-black, &c. [c], and by the experiment above recited, with the cylinder of ivory and the glass &c.; for in this experiment, the charge being resisted by the ivory (which however is sometimes split by the explosion) forces a passage between that and the glass, and being there confined by heavy weights, exerts

(c) See a curious instance of this kind in M. ADANSON's Voyage to Senegal, p. 239.

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its expanſive power in ſuch manner as to reduce to the ſmalleſt fragments the plate of glaſs then expoſed to its violent operation. 2dly, This violent effort of the electricity produces not the leaſt effect upon the ſurface of the ſlip of paper painted with lamp-black and oil, or upon a ſlip of oiled ſilk, placed in the ſame ſituation. 3dly, May not therefore a coating of lamp-black and tar, or lamp-black and oil, be in ſome caſes uſefully applied on ſlight buildings of wood, &c. to preſerve them from damage by lightning, as well as to prevent thoſe large cracks and rents (the uſual effect of the heat of the ſun) from being made in them? 4thly, As the effect of the lightning on the maſts of ſhips has been in ſo many inſtances prevented by a coating of lamp-black and tar, or lamp-black and oil, it ſeems probable, that a ſafe and fixed conductor might be applied to them in a very cheap and convenient manner, as follows; *viz.* let all thoſe parts of the maſt which are uſually greaſed, be provided with plates of metal three inches broad, which plates might extend a few inches upon the other parts of the maſt which are coated with lamp-black and tar, or lamp-black and oil; and thus by the conductor of metal, and the protector of lamp-black and tar, placed alternately and extending the whole length of the maſt, it would probably be preſerved from damage by lightning. A

metallic

metallic communication might be made from the maſt
to the water in the manner I have before mentioned, in
Phil. Tranſ. vol. LXIV. p. 412. This method of making
conductors to ſhips, from its ſimplicity and practicability,
I had ſome thoughts of recommending to my acquaint-
ance in the marine department; but there is one objec-
tion to it, which I think a very material one, and ſhall there-
fore ſtate it in its full force: it is this; the lamp-black and
tar, or lamp-black and oil, though they protect, by their
property of repelling the electric matter, thoſe parts of the
maſt which are coated with them, yet being perfect non-
conductors, thoſe things or perſons which might hap-
pen to ſtand in their vicinity (as in the tops, &c.) would
be in danger of a ſevere ſtroke, perhaps deſtruction, by
the lightning. How far the other oil colours, *viz.* thoſe
prepared from minerals and metals, may anſwer theſe
purpoſes, may perhaps deſerve enquiry, and the more ſo,
as the experiments are not difficult to make. The belfry-
poſts painted with white lead, mentioned in the letter
above recited, were much ſhivered. 5thly, As oiled ſilk
ſeems to be ſo good a ſecurity againſt the effects of a
charge of electricity, may not garments, *viz.* cloaks and
hats, covered with that ſubſtance, contribute in ſome
meaſure to protect the wearers (if overtaken by a ſtorm)
from a ſtroke of lightning? particularly, if the precau-

tions

tions be properly attended to, which I have before re-
commended from Dr. WINTHROP, in Phil. Tranf. vol.
LXIV. p. 151.

P A R T   II.

On the electricity of chocolate: and the restoration of that
property to it, when lost, by melting it together with a
small quantity of olive-oil.

HAVING been informed by my ingenious friend
Mr. GEORGE ADAMS, philofophical inftrument-maker to
his majefty, that Mr. SANDERS, an eminent manufacturer
of chocolate, had frequently obferved a very vivid light
flafhing upon its furface, when cooling in the tin pans in
which it is received from the mill; particularly in clear,
frofty evenings, when it would alfo ftrongly attract light
fubftances, fuch as fmall particles of duft, bits of paper,
ftraw, thread, &c. [a]; I was very defirous to afcertain, if I
could, the caufe of thefe phenomena.   For this purpofe
I waited on Mr. SANDERS, in company with Mr. ADAMS,
and made the following experiments.   1ft, A large cake

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[a] The wax-chandlers alfo, in forming their fticks, &c. of wax, are fre-
quently fpectators of thefe effects of electric attraction.

of

of chocolate being turned out of the tin pan, in which it had been ſet to cool; I preſented towards it Mr. CANTON's electrometer, and obſerved that, at ſix inches diſtance, the balls began to diverge; and when they came within two inches of the chocolate (being ſuſpended over it) their divergence was full an inch and an half, and upon examining their electricity, I found the chocolate to be in a *plus* or poſitive ſtate. 2dly, Having ſeparated another large cake from the pan, I touched it repeatedly with the knob of a ſmall phial, properly prepared for the Leyden experiment; then bringing that knob gently toward one of my knuckles, I ſaw a ſpark between them, and had a ſmall ſenſation in my knuckle. 3dly, Having ſeparated another cake of chocolate from the tin pan which contained it, I touched the cake repeatedly with the braſs ball on the neck of my Leyden *vacuum*, or analyſis of the Leyden bottle[e], and inſtantly perceived a moſt beautiful and large pencil of rays darting from the wire, and ſpreading themſelves through the bulb towards the coated part of the bottle. 4thly, Changing the poſition of the bottle, I preſented the coated bulb towards the chocolate, and then perceived (as I expected) a ſmall luminous ſpark upon the point of the wire in the neck of the bottle; completely proving the electrical quality of the

(e) Deſcribed in Phil. Tranſ. vol. LXIV. p. 400.

chocolate,

chocolate, and afcertaining its direction in the experiments.

Before I had an opportunity of making this complete inveftigation, I had feparated a piece of chocolate from the tin pan in which it had been cooled, and accidentally left, for fome months, under an open counter in a fhop, expofed to duft, damp air &c.; notwithftanding which, on its feparation from the pan, it attracted a thread of trial at a quarter of an inch diftance. I then took a quarter of a pound of chocolate, and having melted it in an iron ladle, poured it into a tin pan, and the next day (it being perfectly cooled) feparated it from the pan, and found it ftrongly electrified *plus*; but as the electricity was foon loft by handling (owing, I fuppofe, to the large quantity of conducting matter contained in it) I melted it again, but produced no electricity; which I imputed to the chocolate having become very dry and powdery. I therefore melted it a third time, adding a little oil of turpentine; but this trial alfo (perhaps from the evaporation of the fpirit) failed. I then melted it the fourth time, and added a fmall quantity of olive-oil, fufficient as I imagined to reduce it again to its original confiftence, and having cooled it in the tin pan as before, I found on removing it, that its electricity was completely reftored. The large proportion of phlogifton in oil is well known; and as the addition of oil to the chocolate

<div align="right">completely</div>

completely reftored its electricity when loft, is not this an indication of a great affinity at leaft between phlogifton and the electric fluid, if indeed they be not the fame thing*(f)*? Further, as electricity is produced in the chocolate by heat and friction, and manifefted by its ufual phenomena in the cooling of that fubftance, *query*, may not electricity be produced from the other oily nuts, kernels, or feeds (particularly thofe of the torrid zone) treated in the fame manner?

However, as the electric matter is refident in, and may be difengaged from, all the fubftances we are acquainted with; as the air is at all times replete with it; as its opeation is fo fecret, fo rapid, and at times fo tremendous; as it is fo eafily excited or put in action by friction, by heating and cooling, and perhaps by means we are totally unacquainted with; I think' we may fafely conclude, that electricity, as it is one of the moft powerful, is alfo one of the moft important, agents in nature. Many ufeful difcoveries have been made refpecting the action, influence, and effects of this fubtile fluid; but certainly much remains to be done, and the field for future labourers' feems daily to enlarge. Indeed, notwithftanding the number of difcoveries in electricity this age may juftly boaft of, I cannot but be of opinion (which I men-

(f) A thick fcum from the furface of fome linfee-doil expofed to the air, and thoroughly dried, became a very ftrong negative electric.

tion as an incitement to the ftudy) that, compared with
the facts ftill undifcovered in that branch of philofophy,
they bear but a very fmall proportion.

P A R T   III.

Obfervations on fome new and fingular phenomena in ex-
cited and charged glafs; with experiments made in con-
fequence of thefe phenomena, further illuftrating the
Franklinian theory of the Leyden bottle; and a defcrip-
tion of the apparatus conftructed for that purpofe by
Mr. HENLY.

HAVING carefully repeated the experiments with
the two coated plates of looking-glafs, mentioned in my
paper on Mr. VOLTA's machine, and finding with Mr.
LANE, that they exactly agreed with the account given
by Mr. SYMMER and Mr. EELES, I was defirous to be fa-
tisfied whether glaffes of a different thicknefs would be
differently affected in the experiment. For this purpofe
I tried two large fquares of crown or window-glafs, and
found them to charge and difcharge exactly as the look-
ing-glafs plates had done; but on trying the experiment
with two plates of Nuremburg glafs, commonly called
Dutch plates, I was not a little furprifed to find that each

of

of the plates, when feparated after charging, had a pofi-
tive and a negative furface; and that having replaced
them, and made the difcharge as in the Leyden experi-
ment, the electricity of all the furfaces was changed,
though it appeared to be very ftrong, and continued to
give repeated flafhes of light, when the plates were al-
ternately clofed, touched, and feparated, as the looking-
glafs plates above mentioned. If a clean dry uncoated
plate of looking-glafs was placed between the coated
plates, either of looking-glafs or crown-glafs, before they
were charged; that uncoated plate was always found,
upon feparating them after charging, to be electrified
negatively on both its furfaces; but if it was put between
the Dutch plates, it acquired, like them, a pofitive and a
negative electricity. As this phenomenon was not fatif-
factorily accounted for, it occafioned much converfation
with refpect to Dr. FRANKLIN's theory of the Leyden
bottle, which I had myfelf (as I imagined) fatisfactorily
explained and even demonftrated. I was, however,
foon convinced, that that theory is not fo generally
received as I imagined; for I met with a number of
gentlemen who not only doubted, but feemed abfolutely
to deny it. This induced me to make fome further ex-
periments, in order (if I could) more fully to illuftrate
that theory, and to put the matter out of doubt. For

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this purpose a pretty large jar was coated and furnished as in fig. I. A is the jar; BB the tin-foil coating; C a tin-stand which supports the jar; D a socket of metal which supports a rod of glass E; F a curved wire or plate of metal with points, not very sharp; this wire or plate of metal is fastened to the end of a brass rod G, which rod is moveable at pleasure in a spring tube H, that tube being fixed by a socket upon the top of the glass rod E. The charging wire of the jar communicates with both parts of the inside coating of the jar by horizontal wires (the ends of which are bent a little downwards) fixed at right angles to each other, in order to prevent shaking and ratling.

THE USE OF THE DOUBLE COATED JAR.

According to Dr. FRANKLIN's theory, the same quantity of the electric matter which is thrown upon one of the surfaces of glass in the operation of charging it, is at the same time repelled or driven out from the other surface, and thus one of the surfaces becomes charged *plus*, the other *minus*; and that this is really the case is, I think, satisfactorily proved by this contrivance. For example, place the jar as usual, with the knob in contact with the prime conductor; then work the machine, and the apparatus being perfectly dry and in good order, a small luminous spark will appear upon the upper point of the wire F (a plain indication that the point is then receiving

electricity

electricity from the upper ring of coating on the outſide of the jar) and a fine ſtream or pencil of rays will at the ſame time fly off, beautifully diverging from the lower point of the wire F upon the bottom ring of coating on the jar. When theſe appearances ceaſe, which they will as ſoon as the jar becomes charged, let a pointed wire be preſented towards the prime conductor; this will ſoon diſcharge the jar ſilently, during which the lower point of the wire F will be illumined with the ſmall ſpark, while the upper point of the wire will throw off a fine pencil of rays, diverging towards the upper ring of coating, to which it ſtands contiguous, as upon Dr. FRANK-LIN's hypotheſis it ought to do. A wire of the ſame form as that marked F may be inſerted on a ſmall electric ſtand, fitted by a proper baſe to the bottom of the jar on the inſide; this will ſhew the appearances when the jar is charged negatively.

The ſame experiment may be very conveniently made with a large pane of crown glaſs [g], coated in two places at a proper diſtance from each other (fig. 2.) leaving a ſufficient quantity of glaſs uncovered quite round the two coatings. This coated plate of glaſs ſhould be fixed in a frame, and mounted upon a proper electric ſtand. Another ſtand of glaſs or ſealing wax ſhould be provided,

[g] Mine is eighteen inches by fourteen inches.

to support a wire or piece of metal placed horizontally, and curved so as to bring the ends of it, which should have blunt points, within half an inch distance of the two tin-foil coatings on one of the surfaces of the glass. On the opposite side of the glass, two wires, bluntly pointed, are also to be employed; one of these is to communicate with the prime conductor, and to throw off the electricity from thence upon one of the coatings of tin-foil placed contiguous to it; the other wire is to communicate with the earth, standing in a perpendicular direction, with the point bent towards and reaching within half an inch of the other coating of tin-foil (on the same surface of the glass) to receive the electricity thrown off by that coating, while the opposite side is charging.

EXPERIMENT.

The apparatus being perfectly dry (the uncoated part of the glass and the frame, &c. should be varnished), clean, and in good order; the plate of glass should be so fixed, that each of the four coatings of tin-foil may come within half an inch of the point of the wire opposed to it. The apparatus being thus placed, if a powerful machine be worked in a dark room, the electricity will be seen to issue from the point of the wire in contact with the prime conductor upon one of the tin-foil coatings A

(fig.

(fig. 3.) charging it poſitively. The coating B (fig. 3.) on the other ſide of the glaſs throwing off, at the ſame time, an equal quantity of the electric matter, (viſible in the form of a ſmall luminous ſpark upon the point of the inſulated wire F) is thus left in a negative ſtate. The electricity paſſing along the inſulated wire, flies off from the other point of it in a pencil of rays, diverging upon the tin-foil coating C (fig. 2.) on the ſame ſide of the glaſs, charging it poſitively; while the oppoſite coating D (fig. 3.) throws off its electricity, which is received in a ſmall ſpark upon the point of the wire (G) oppoſed to it, and communicating with the earth. Thus, by the ſame operation of the cylinder, may a poſitive and a negative charge of electricity be obtained at the ſame time upon each ſurface of the glaſs; and by applying two curved diſcharging rods (which ſhould have glaſs handles) at the ſame inſtant, ſo as to come nearly into contact with the coatings upon each ſurface of the glaſs, the whole will be diſcharged together; or if a pointed wire be preſented near to the prime conductor, they will all be diſcharged ſilently, and then the appearances on the points of the wires will all be reverſed; that which was a bruſh or pencil of rays being now a ſmall luminous ſpark, and that which was a luminous ſpark being a bruſh or pencil of rays. If the machine be very powerful, the

rubber may be infulated, and a blunt pointed wire, com-
municating with the earth, may be placed within half
an inch of it; this wire, while the plates are charging,
will throw off a beautiful pencil of rays diverging upon
the rubber, and thus compleatly exhibit the progrefs of
the electricity through all the apparatus, from its exit
out of the earth to its entrance into the earth again: and
its return may be manifefted by reverfing all the ap-
pearances upon the points of the wires, in the operation
of difcharging the glafs filently by a pointed wire pre-
fented toward the prime conductor, as above directed.
Another very fatisfactory method of demonftrating the
truth of Dr. FRANKLIN's hypothefis is as follows. I take
a bottle, containing about one hundred fquare inches of
coated furface, properly prepared for the Leyden expe-
riment, and holding it by the wire, I fet the coating upon
the prime conductor, and charge it negatively (fig. 6.);
when charged (if not too dry) the upper edge of the
coating will throw off one or more pencils or brufhes of
light into the air, which vifibly incline towards the
charging wire of the bottle, and fometimes actually reach
it. If I hold the bottle by the coating, and prefent the knob
to the prime conductor, charging it pofitively (the bottle
being in a proper ftate) a fmall fpark of light firft appears
upon the edge of the cork in the neck of the bottle, through
which

which the wire paffes; after a few turns of the globe, this fpark becomes a fine brufh, darting out from the cork, and gradually lengthening, till it forms a beautiful arch, the end of it regularly extending downward, till it reaches the edge of the coating and refts upon it (fee fig. 5.). I remember, when I firft fhewed thefe experiments to my fincerely refpected and worthy friend the late ingenious Mr. FERGUSON, F. R. S. he expreffed great fatisfaction; and affured me, that he thought them fome of the moft convincing he had ever feen exhibited for the purpofe. If the bottle be dry, it will, in both cafes, be difcharged fpontaneoufly; but if the uncoated part of the glafs be then breathed upon, the appearances may be produced at pleafure. I have lately prepared another bottle for this purpofe, the infide of which is coated in the ufual manner; but the outfide is covered with fquare pieces of tin-foil about a quarter of an inch broad, and about three-fixteenths of an inch diftant from each other; the bottom is compleatly covered with the coating (fig. 4.). If in charging this bottle, the electricity paffed abfolutely through the glafs, it would find a ready conveyance by the coated bottom into the table (and then indeed it could never be charged at all); but the truth is, that this bottle does not become charged till ftrong flafhes of electricity have paffed,

diverging in different directions quite round it. If it be discharged by bringing a pointed wire near the wire, or the prime conductor, with which it is in contact, the noise it occasions much resembles the report of a fired cracker; and the uncoated glass between the spots of tin-foil is very brilliantly illuminated. If the bottle discharges itself spontaneously, or be discharged suddenly, by making a regular communication by the rod between the two surfaces of the glass, the whole outside surface seems to be illuminated. To produce these appearances the glass must be thoroughly dry.

EXPERIMENT.

A smooth piece of mahogany, two inches square and five inches long, was hollowed into an elliptic groove, about three-quarters of an inch deep, and painted with lamp-black and oil. Into this groove two wires, terminated by brass balls each three-quarters of an inch in diameter, were introduced; the brass balls being placed about one inch and an half from each other: between the brass balls, at an equal distance from each, was placed a ball of the pith of elder, half an inch in diameter, nicely turned in a lathe. The apparatus being thus adjusted, and the circuit compleated by a short chain, a bottle, containing forty square inches of coated surface,

was many times difcharged through it; and whether the bottle was charged pofitively or negatively, the pith-ball conftantly moved in the direction of the fluid, according to Dr. FRANKLIN's hypothefis. This is a delicate experiment; but, as I have perfectly fucceeded in it, I thought it proper to give this fhort account of it. Perhaps if the pith-ball were fufpended by a filk ftring, it might anfwer the purpofe as well; but this I have not tried.

EXPERIMENT.

In melting fmall wires fome inches in length, I have often obferved the wire to become red-hot, firft at that end in contact with the difcharging rod, and the rednefs has proceeded gradually and regularly towards the coating of the jars or battery; plainly and fully demonftrating the direction of the electric matter in the difcharge of the jars or battery, which, for this experiment, were always charged pofitively. This phenomenon hath alfo been obferved by Mr. BELL, and many times by Mr. NAIRNE.

EXPERIMENT.

Before I quit the fubject of the Leyden bottle, I fhall mention one experiment more which I have lately made, and which gives a full and compleat anfwer to a paragraph in Dr. PRIESTLEY's Hiftory of Electricity, 2d edit. p. 465. l. 21. It is this: I procured fome phials coated with fil-

ver,

ver, by burning it into the very fubftance of the glafs, in. fuch a manner that nothing can remove it without injuring the glafs together with the metal. Glafs thus coated and burnifhed has certainly a moft elegant appearance, has no inequalities or points upon its furface, and charges as high and as readily as when it is coated with tin-foil; fuch glaffes will difcharge fpontaneoufly, and one of them, which was very thin, was burft by the explofion; an acci-dent which, by this double annealing, I was in hopes to have prevented, but was forry to find myfelf difappointed.

OBSERVATIONS ON EXCITED GLASS.

In my remarks on Mr. VOLTA's curious little machine lately prefented to the Royal Society, I have obferved that the electric matter, being once thoroughly excited and put in action, is not fo foon as might be expected reduced again to a quiefcent ftate, efpecially in bodies fo peculiarly adapted to affect each other as thefe appear to be. As a proof, I introduced the experiments with the looking-glafs, crown-glafs, and Dutch plates. I alfo men-tioned Dr. PRIESTLEY's obfervations on the *refiduum* of his battery; and in a note to that paper, I added an ex-periment made by my friend the rev. Mr. HEMMING, F. R. S. who fhewed me a fmall bottle, which then at-tracted a thread of trial, though it had ftood in a cup-

board

board in his ftudy feventy days from the time he charged
it. I alfo mentioned a fingular effect of his cylinder,
which will feparate the balls of Mr. CANTON's electro-
meter, at twelve or fourteen inches diftance, fometimes
a fortnight after ufing, though the air of the room may
have been many times changed, and a variety of methods
ufed to deftroy that power in the interval. As the detail
is curious, I fhall here infert one fet of experiments,
as I find them regiftered in a journal, which Mr.
HEMMING took the trouble to make for my fatisfac-
tion. On May 13 1776, the cylinder was ufed, and
when placed in the cupboard at 10 o'clock A. M. it fepa-
rated the balls at feven inches diftance. The power was
then entirely deftroyed by breathing on it, and the elec-
tricity marked o. From that time the journal proceeds
as follows:

Day.	Hour.	Diftance at which the balls diverge.
May 13	11 A. M.	7 inches.
14	8 A. M.	13
Breathed on it once,		9
Ditto four times,		8
Door open till	9½ A. M.	5
14	3 P. M.	7
	7 P. M.	6
	10 P. M.	6½

Breathed

Day.	Hour.	Diftance at which the balls diverge.
Breathed on it twice,		o inches
May 14	10½ P. M.	6
15	8 P. M.	13 wind N.
Door open ten minutes,		8
	9 P. M.	13
	10 P. M.	8
The power deftroyed by flame,		o
16	7 A. M.	14½ wind N.
18	8 P. M.	17 wind N.
	11 P. M.	7½
Deftroyed by flame,		o
20 A fire in the room the whole day.		
	4 P. M.	2
	8 P. M.	1
	10 P. M.	3
21	7 A. M.	9
	9 P. M.	9
Deftroyed by flame quite round		o
22	7 A. M.	8½
	10 A. M.	11½ wind N.
Applied flame quite round the cylinder,		o
	1 P. M.	12½

May

Day.	Hour.	Diſtance at which the balls diverge.
May 22	4 P. M.	12½ inches.
	8 P. M. { flame quite round. }	0
	10½ P. M.	2

23d, Mr. HEMMING ſhewed me the experiment, when I ſaw the balls ſeparate at twelve inches diſtance from the cylinder. The cauſe of theſe phenomena is, no doubt, the excited electricity lodged in the pores of the glaſs acting upon the vapour in the air of the room, and producing a greater or leſs effect, as circumſtances may contribute to increaſe or diminiſh its operation. The cylinder was now uſed again, which cloſed this ſet of obſervations. Mr. HEMMING has added a meteorological journal for the time; but the particulars of this I did not think it neceſſary to introduce in the above extract.

A SECOND SET OF OBSERVATIONS.

Feb. 3, 1777, the cylinder was excited, and from the 5th to the 14th no fire had been made in the ſtudy.

Day.	Hour.	Diſtance at which the balls diverge.
14 [b]	3 P. M.	7 inches.
	10 P. M.	0
15	9 A. M.	11

[b] This was the firſt time, ſince Auguſt or September, that I had obſerved my cylinder to retain its attractive power longer than twelve hours after being excited, though it was conſtantly kept in the ſame place, and, as well as I can obſerve, under the very ſame circumſtances.

Feb.

Day.	Hour.	Diſtance at which the balls diverge.
Feb. 15	4 P. M.	8 inches.
	9 P. M.	0
	10 P. M.	0

A large fire in the ſtudy the whole day.

16	9 A. M.	9
	12	9
	8 P. M.	0
	10 P. M.	0
17	9 A. M.	3
	6 P. M.	2
	11 P. M.	0
18	$8\frac{1}{2}$ A. M.	2
Abſent till 22d,	3 P. M.	8
	6 P. M.	2
	7 P. M.	0
	11 P. M.	0
23	8 A. M.	7
	2 P. M.	2
	5 P. M.	0
	9 P. M.	0
	11 P. M.	0
24	9 A. M.	0
	12	0
	3 P. M.	0

Feb.

Day.	Hour.		Diftance at which the balls diverge.
Feb. 25	9	A. M.	2 inches.
Abfent till 28th,	3	P. M.	2
	9	P. M.	0
Mar. 1	9	A. M.	0

The fire in the ftudy put out at noon.

	10	P. M.	0
2	9	A. M.	0
	1	P. M.	0
Abfent till 8th,	3	P. M.	$9\frac{1}{2}$
	6	P. M.	4
	8 9 }P. M. 10		0
9	8	A. M.	9
	7 8 }P. M. 10		0
10	8	A. M.	8

The cylinder was now excited again, which clofed this fet of obfervations. Thefe changes in the electricity feem very extraordinary, and I think them not eafily to be accounted for, as they happened in ftates of the weather which were totally different. I regret, however, that an accurate and fenfible hygrometer was not obferved the

whole time; and for fuch a purpofe I fhould recommend Mr. COVENTRY's, made with a number of circular pieces of iffue-paper, amounting to a certain weight, thoroughly heated and ftrung on a thread, kept feparate from each other by a fmall glafs bead, and fufpended on one of the ends of a lever, nicely poized, and turning freely on its axis; the other end ferving as an index to a graduated fcale, on which it fhews the weight of the moifture imbibed at any time by the papers. This hygrometer, from its extreme fenfibility, I fhould choofe, I fay, to recommend for this purpofe, in preference to any other that I have ever feen.

HAVING lately had occafion to fhew the experiments with the coated Dutch plates to an excellent electrician (Mr. CAVALLO) and having charged them as high as I could, and feparated them I think rather more expeditioufly than ufual, I was aftonifhed to find, that the very fame plates I have fo often mentioned were now charged, the one pofitively, the other negatively, on both furfaces. I then laid them together, and having made the difcharge as ufual, I feparated them, and found one of the plates negative on both fides, and the other plate pofitive on one furface, and negative on the other. Here was a new caufe of admiration, and I was utterly at a lofs to account for it, as the plates had in every inftance be-

fore uniformly acted as repreſented in my paper. At length I recollected, that this experiment had been made rather more expeditiouſly than uſual: I therefore repeated it, and having allowed ſomewhat more time between the removal of the plates from the prime conductor, and the ſeparation of them, in order to examine their electricity, I found on each plate a poſitive and a negative ſurface; and having replaced them and made the diſcharge, I obſerved that the electricity of all the ſurfaces was changed. I have mentioned this circumſtance, to ſhew how ſmall a difference in the manner of making an experiment, will make an eſſential difference alſo in the reſult. There is ſomething, however, very ſingular in this kind of glaſs, which I believe is owing to its not being properly annealed; for I once met with a plate of it which I found very difficult to charge at all; and when a ſmall quantity of electricity had been forced into it, it diſſipated proportionably ſooner, without the uſe of the diſcharging rod.

In glaſs, properly annealed, whether in the form of plates or jars, prepared for the Leyden experiment, the diſſipation of the electricity is, in ſome ſtates of the atmoſphere, a remarkable, and ſometimes (when there is not a fire in the room) a diſagreeable circumſtance: this effect, however, in the jar itſelf, may be in a great meaſure prevented, by having the uncoated part of the glaſs

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neatly

neatly covered with the beſt varniſh [g]; and I ſhould not omit to obſerve, that Mr. HEMMING's bottle, which retained its charge ſo long, was prepared in this manner.

P O S T S C R I P T,

Containing ſome experiments and obſervations on Mr. VOLTA's *machine, by* Mr. TIBERIUS CAVALLO, *with remarks by Mr.* HENLY.

Mr. VOLTA's machine, which occaſioned ſeveral of the preceding enquiries, hath lately been made by Mr. CAVALLO, by coating the glaſs plate (about ſix inches in diameter) with ſealing-wax. With one excitation of this plate he ſoon charged a bottle compleatly, and with that charge pierced three holes in a card, which he hath ſince ſhewed me. If, when this machine acted vigorouſly, he inverted the excited plate, and ſet the braſs plate upon the glaſs, he produced a contrary electricity, but in a much ſmaller degree. If when the ſealing-wax was ſtrongly excited, ſo that ſparks, two inches long, might be drawn from the braſs plate, the excited wax, &c. was placed on an electric ſtand, and the proceſs continued as uſual; the ſparks from the braſs plate preſently dimi-

[g] The varniſhing ſhould be ſeveral times repeated.

niſhed,

nifhed, and in a fhort time almoft totally ceafed: this, I think, clearly indicates that the electricity in the lower furface of the glafs and the table were mutually affected in the operation, as well as that, in the excited fealing-wax and the brafs plate[h]. I have feen one of thefe machines, made by Mr. CAVALLO, act fo ftrongly that, upon fepa-rating the brafs plate from the fealing-wax, a flafh has ftruck from the brafs toward the table, and it has befides given a ftrong fpark upon the knuckle, when held at up-wards of an inch diftance. If the brafs plate, after being raifed from the wax, be prefented with its edge toward the wax (lightly touching it) and thus drawn over its furface, the electricity of the plate, he obferves, will be abforbed by the fealing-wax, clearly fhewing the ftrong negative ftate in which the excited wax is left on the re-moval of the brafs plate.

The attraction between the plates is alfo fometimes fo ftrong, that the coated glafs has frequently been lifted up by the brafs plate from the table; yet in a few days, being carefully placed in a proper repofitory (in contact with each other) not the leaft fign of electricity has been

(h) It has been fuppofed by fome gentlemen, that the very fame quantity of electricity imparted by the finger to the plate on touching it, was emitted again by the plate on removing it from the electric and prefenting it towards the knuckle; and that therefore, in air perfectly dry, this machine would at all times exhibit its phenomena without a frefh excitation of the electric, and thus merit the appellation of a machine for exhibiting perpetual electricity: but the fact above mentioned entirely refutes that fuppofition.

difcoverable

difcoverable on their feparation: fo far is even this ma-
chine from exhibiting perpetual electricity.   Indeed, in
this particular, it is far exceeded by Mr. GREY's apparatus
of the cone of fulphur in the glafs, which, on being fe-
parated, I have never perceived to fail of exhibiting
ftrong figns of electricity, in every ftate of the weather.
To this apparatus I have lately added the improvements
of M. ÆPINAS, and find that they fully anfwer his report.

THE following paper contains a fet of experiments
which may perhaps lead to fome curious, ufeful, and im-
portant truths in electricity.   They are made with the
moft fimple apparatus, and in the moft fimple manner;
nothing more being requifite for this purpofe than a few
fticks of fealing-wax (one of them being referved as a
teft) to the ends of which the fubftances to be examined
are to be fixed or tied as occafion may require, and Mr.
CANTON's electrometer, neatly made and properly infu-
lated.   With this fmall apparatus may almoft every ar-
ticle that can be propofed be examined with the utmoft
facility.   The animal, the vegetable, the foffil kingdom,
with all the works and combinations of art and manu-
facture, may afford materials; almoft any of which, by
a flight friction againft woollen cloth or filk, will become

electrified

electrified (either poſitively or negatively, according to the nature of the ſubſtance and form of its ſurface, and the quality and ſurface of the rubber) ſufficiently to ſe-parate the balls of the electrometer, ſo as to determine their electricity in a very ſatisfactory manner. This, I think, fully confirms an opinion I have long entertained and mentioned in a former paper, *viz.* that the ſlighteſt friction of bodies of every kind, in every ſituation, may diſturb the electric matter contained in them, though the effect be imperceptible to us, having no electrometer nice enough to diſcover it. Here, therefore, is a boundleſs field for future enquiry; and, to aſſiſt thoſe who may be inclined to proſecute it, I ſhall mention a few general ob-ſervations and precautions, which I have found exceed-ingly uſeful in the courſe of my own experiments. 1ſt, The air ſhould be dry, and the apparatus clean and warm. 2dly, The ſubſtances to be tried ſhould be perfectly clean. 3dly, When the rubber hath been uſed once or twice, it ſhould be held near the fire or the flame of a candle, not only to prevent its acquiring moiſture, but to take off the electricity left in it by one ſubſtance, before another be examined; for it ſhould always be remembered, that whenever a ſubſtance is made electrical by friction, the rubber acquires the contrary electricity, and this electri-city, if it be not carefully taken off as is above directed,

will

will sometimes remain in it so as to confuse and actually mislead in the experiments. 4thly, Some minute substances, as a small leaf, seed, or hair, will not be easily excited in damp weather; these, therefore, as well as the apparatus, should be warmed; for heat, I find, doth always dispose bodies to become electrical. 5thly, The insulating stands, and the sticks of sealing-wax, with which the substances to be examined are connected, should not be rubbed, lest they, by the friction, should be made electrical, and, acting through the substance in contact with them, deceive in the experiment. 6thly, The animal substances, as hair, horn, bone, cartilage, nails, teeth, muscles, &c. become electrified positively, by friction, against woollen cloth or black silk; and the vegetable creation, with very few exceptions, negatively. The metals differ with respect to kinds, form, and surface, and may be differently affected by different rubbers. Lastly, I must not omit to observe that, among vegetables, I find the hot, acrid, pungent, and aromatic substances, as the spices, &c. to be much more easily excited, and stronger in their power, than the cold ones, as the seeds of gourd, melon, or cucumber. Among the herbs, hemlock and parsley are strong: a single leaf of laurel, bay, yew, rosemary, &c. will be found very powerful; but, as I have before observed, this field is indeed immense, and life itself too

short

ſhort for a compleat inveſtigation. The experiments prove, however, how univerſally the electric matter is diſſeminated; or, in other words, its exiſtence in all bodies; with what readineſs it is excited; and, I think, the conſtancy of its action (though imperceptible to us) as well as its uſe and importance as a principal agent in the greateſt, and to mankind the moſt intereſting, operations in nature. I have encloſed a catalogue, exhibiting at one view the articles I have had an opportunity to try, with their kinds of electricity marked againſt them; hoping this ſpecimen may induce gentlemen of more leiſure to purſue the enquiry.

Hair, wool, down, and many other articles, may be made up in the form of little taſſels, and in this manner readily fixed upon or tied to the end of an electric for experiment.

The following fubftances being fixed or tied upon the end of a ftick of fealing-wax, and excited by friction againft a woollen garment or a piece of foft black filk, became electrified as marked in the columns of the annexed table. The ftrongeft in power are diftinguifhed by the letter s, and the weakeft by the letter w.

M E T A L S.

	Wool.	Silk.
A new guinea; a fmooth fix pence; a brafs ferule; tin, and tin-foil; enamelled coppcr, s; gilding on leather, s; lead ore; copper ore; iron ore; ftream tin;	Neg.	Neg.
Milled lead; copper, s; a polifhed fteel button, s; a new filver ditto; a metal button gilt, s; tutenague ditto, s; iron;	Pof.	Pof.
Lead from a tea-cheft, in which there is a mixture of tin, w;	Neg.	Pof.
A gilt button, bafket pattern; the juncture at the end of a brafs ferule;	Pof.	Neg.

A N I M A L

ANIMAL SUBSTANCES.

	Wool.	Silk.

Tortoife-fhell, w; ivory, s; bone, s; horn; lamb's-tooth; horfe's-hoof; deer's-hoof; mufcle of the leg of a deer, s; cartilage, s; fpur of a young cock; bill, claw, and fcale from the leg of a turkey, s; fcale of a carp; the *chryfalis* of a moth, recent from the earth, cleanfed; *craffamentum* of the human blood exficcated, w; quills; claw of an unboiled lobfter; cowrie and feveral other fmooth fhells, s; fhell of a hen's egg; tail of a fmall fifh; thigh of the elephant beetle; a fmall beetle, fmooth furface; human hair; red and white horfe's and bullock's hair, s; hog's briftles, s; wool; filk from the worm, w; oyfter-fhell, fmooth furface; } Pof. Pof.

Mother of pearl, and feveral other fhells; } Neg. Pof.

Mufcle and cockle-fhells, recent; a recent fnail-fhell, rough furface; *elitra* of the ftag-beetle; oyfter-fhell, rough furface; } Neg. Neg.

VEGETABLES.

Wool. Silk.

Rind of cheftnut, s; Barcelona nut-
fhell, s; cafhew nut, s; cocoa nut-fhell
polifhed; brazil; *lignum vitæ*; black
ebony, s; box, w; cane, s; *quinquina,*
or Peruvian bark, s; tamarind-ftone;
coffee-berry roafted, s; nutmeg, s; gin-
ger, s; white pepper, freed from the
hufk, s; cinnamon, s; cloves, s; mace, s;
all-fpice, s; capficum, both fides of the
pod, s; hemlock, s; a clove of garlic;
ditto of efchalot, freed from the hufk, s;
a green onion, s; rue, s; cork, s; leaves
of laurel, bay, yew, holly, rofemary, with
their berries, s; parfley, s; leaf of tur-
nip; ditto of Savoy cabbage, s; celery, s;
fago, s; thime, s; carrot; turnip; potatoe;
an acorn, s; rind of Seville orange, s;
a large Windfor bean, s; a white pea;
root of the white lily; fnow-drop root;
feeds of gourd, melon, cucumber, w; a
fpecies of long-mofs, w; an apple, s;
down of the cotton-rufh, w; fea-flag;
leaf of the American aloe, s; cotton, w;

Neg. Neg.

Hemp;

	Wool.	Silk.
Hemp; flax; ſtalk of the tobacco-leaf; ſpike, from the leaf of the American aloe; *palma-chriſti* nut; horſe-radiſh;	Neg.	Poſ.
A white kidney-bean, ſmooth ſurface; black negro of the ſame; ſcarlet of the ſame;	Poſ.	Poſ.

CORALLINES.

	Wool.	Silk.
Sea-fan, the horny part, w; rough coral, w;	Neg.	Poſ.
Spunge, w; coral poliſhed, w;	Poſ.	Poſ.

SALTS.

	Wool.	Silk.
Allum, w,	Neg.	Neg.
Borax, Nitre purified, } ſmooth ſurfaces;	Poſ.	Poſ.

FOSSIL AND MINERAL SUBSTANCES.

	Wool.	Silk.
Common pebble-ſtones of all colours, s; marble, s; pit-coal, s; black-lead, w; jet, s; *aſbeſtos*; mineralized ſulphur; thunder-bolt ſtone; *cornu-ammonis*; ſhark's-tooth; coat of petrifaction;	Neg.	Neg.
Several ſmooth native cryſtals; brown Iceland ditto; *talc*, s; Ceylon pebble, ſmooth and tranſparent; agate, s; cornelian; amethyſt, s;	Poſ.	Poſ.

A ſpecimen

	Wool.	Silk.
A specimen of *gypsum*,	Neg.	Pof.

ARTIFICIAL SUBSTANCES.

Staffordshire ware glazed; China ware, s; Wedgwood's ware glazed, s; whale's fin prepared, w; writing-paper; parchment, s; sheep's gut,	Pof.	Pof.

Tobacco-pipe, s; Wedgwood's ware unglazed; elastic gum, s; hard undercrust of a leaf; a tallow-candle, w; oiled silk; painted paper, s; silver, burnt into glass, unburnished; pearl-barley, w; Indian ink, w; blue vitriol, s,	Neg.	Neg.

Dr. LEWIS's glass porcelain,	Neg.	Pof.

Silver burnt into glass, burnished, could not be excited either with the woollen or silk. It is supposed that the substances being so intimately incorporated or blended together, the friction at the same time excited both the substances, so as to counteract and destroy the effect of each other.

In these experiments I have been assisted by Mr. CAVALLO and Mr. ADAMS, who have carefully repeated them in my presence. It may be proper to observe also, that the white pea, a scarlet bean, and the thorn from

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the American aloe[b], being rubbed upon new, fine, claret-coloured cloth, became weakly electrified pofitively.

A lock of black hair from a young colt I have obferved to become very weakly electrified negatively, when excited either with wool or filk.

It will be proper to obferve, that the rubber of woollen cloth, which I ufed in the preceding experiments, was a part of the coat or waiftcoat which I happened to wear at the time; but being defirous to try the effect of another, I took a piece of fine, new, white flannel, and ufing that fide of it which is free from knap, I perceived a remarkable difference; *viz.* the copper and other buttons, the different ftones, fhells, China ware, moft of the animal fubftances (hair excepted) and all the vegetable ones, which with the former rubbers were pofitively, being now negatively electrical; but thofe which were negative with them, I found to be negative with thefe alfo[i]. In all the experiments with the flannel, I had hitherto

(b) All thefe fubftances have fmooth furfaces.

(i) On this account an ingenious friend of mine propofed the following queftion, *viz.* Whether, if the electric matter were inherent (as I afferted) in the different fubftances I examined, the kind of electricity could be changed by the ufe of different rubbers? I reminded him of Mr. CANTON's experiments with a glafs tube, and informed him, that a ftick of fealing-wax becomes pofitively electrified by dipping it in quickfilver, or exciting it with a flip of tinfoil; that a fmooth glafs tube may be made negative by drawing it crofs-wife over

hitherto warmed the fubftances, but kept the rubber cold; fufpecting that, in fome cafes, the refult might be different from what it is when the rubber is warm and the fubftance cold: but this, with the effect of cloths of different textures and colours, filks, fattins, velvets, leather dreffed in oil, and in allum, &c. ufed as rubbers, I have not had leifure to determine. A fmall turnip and a potatoe, which I could not excite at all with either of the rubbers when they and the fubftances were cold, I excited in a very fmall degree with the flannel a little warmed; but very ftrongly when the flannel and the refpective articles were, each of them, moderately warm. A fprig of celery acted very powerfully when the flannel only had been previoufly warmed [k]. As in all cafes the rubber is affected with an electricity contrary to that of the fubftance rubbed, it will be fufficient for

over the back of a cat, or by exciting it with a dry, warm rabbit's fkin; that a fmall coated bottle may be charged with a flip of writing-paper, excited by drawing it brifkly between the fingers of a dry hand fo as to pierce a hole in a card; that the dry leathern cover of a book may be made ftrongly electrical by the friction of a dry hand, and that its electricity is remarkably fhewn by touching it with an infulated button, in the form of the plate to Mr. VOLTA's machine. He then acknowledged, that fuch an objection as he had ftarted muft certainly be groundlefs.

(k) A *palma-chrifti* nut was excited very ftrongly with the flannel, weakly with my coat, and not all (in a room where there was no fire) with the black filk. I have alfo to add, that fome particular fubftances, though negatively electrical when heated, become pofitive when cold, by friction againft the very fame rubber.

many

many purposes to use a rubber with a handle of glass, wax, &c. with which the electricity of many fixed bodies, gems that are set in metals, &c. and other articles that it would be improper to divide or remove, may be determined. Such rubbers of different forms have been constructed and satisfactorily employed in a variety of instances by Mr. ERRINGTON and Mr. CAVALLO, who have extended their enquiries far beyond the limits at which I desisted; their collection of animal, vegetable, fossil, and artificial substances, added to my catalogue, amounting to almost one thousand articles.

C O N C L U S I O N.

AT the conclusion of the second part of the preceding paper, in my remarks upon the electricity of chocolate, I have observed, that many and great discoveries have been made in this age, respecting the action, influence, and effects of electricity; but it is a question that hath been frequently put to electricians, What is electricity? For my own part, I have generally chosen (perhaps for want of a better answer) to reply by a similar question, *viz.* What is air? or, what is water? For, as these are understood to be fluids distinct from all others, and

distinguished

diftinguifhed by the names they bear, fo have I ever con-
fidered electricity as a fluid *fui generis*, and properly cha-
racterifed by the term electricity, electric fluid, or electric
matter; and have always avoided the term electric fire,
as conveying a confufed idea of actual inflammation,
burning, &c.: but I now begin really to doubt, whether
another appellation might not be applied with greater
propriety; whether electricity may not be confidered as a
pure, ethereal, elementary fire, inherent in all bodies, inti-
mately connected or blended with an earthy or other bafe,
and apparently, though not actually, remaining in it in a
quiefcent ftate, till roufed into action by fome proper ap-
plication, as motion, or rather friction, which may, and
probably does, collect it in our experiments. (But can
motion convey inftantaneoufly that which is not material,
but only a quality, a property, an accident, or affection,
of matter, through fuch circuits as thofe of Dr. WATSON,
and produce fuch aftonifhing effects at the interruption
of thofe circuits? Befides, in Dr. FRANKLIN's moft curious
and decifive experiment of charging the Leyden bottle
with its own electricity, the glafs undergoes no friction
whatfoever; but the electricity inherent in it is.fimply ex-
haufted from one of its furfaces, and forced round upon
the other by the electrical apparatus: the fame may be
afferted of bodies prefented toward a conductor negatively

electrified,

electrified, or to the infulated rubber of the electrical machine.) That it may be faid to refide in vegetables, and is extracted together with their oil; that in fermentation, effervefcence, and putrefaction, it flies off in the phlogiftic vapour thence arifing (fee note 1. at the conclufion of this paper); that in diftillation it is difengaged and brought over in an ardent fpirit, in which it refides, retaining its original properties in a purer bafe; that, fince by the collifion of flint, fteel, &c. actual fire is inftantaneoufly produced (as in the inftance of the dry axle of a carriage, which, by the friction of the nave againft it, foon takes fire) fo by the friction of other bodies, which by long perfeverance would produce the fame effect, this latent fire may be firft excited, and its appearances, though unobferved, be thofe we term electrical. A wind-mill, when it works under the break (as the millers term it when no iron is concerned) foon catches fire (the mill-ftones, when no corn is between them, produce the fame effect, though the motion be the fame in both cafes) and many a mill hath been confumed by this means. The method ufed by the Indians, of producing fire by the friction of two pieces of wood againft each other is well known; and in all thefe cafes may not the firft effects of the latent fire, thus roufed into action, be the production of thofe very appearances we call electrical? See notes 2. 3. 4. 5. and 6.

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This thought, I confefs, remained fo ftrongly im-
preffed upon my mind, that I requefted fome of my
friends, who had a better opportunity than myfelf,
to make the trial. For this purpofe fome pieces of
wood were baked in an oven, in order to expel the moif-
ture, and prepare them for the experiment. When they
were cooled, a friction was begun, which, as I expected,
foon produced electricity; one piece of the wood being
excited pofitively, the other negatively, as I have fince
myfelf feveral times experienced. Had the friction been
continued, the production of actual fire might perhaps
have been the confequence. May not, therefore, the
production of actual fire be the *ultimum* of electricity?
or, in other words, electricity the firft effect of latent fire
thus roufed into action; actual fire, the fecond; and in-
flammation and diffolution, its third and greateft effort?
like fermentation, producing firft, wine; fecondly, vine-
gar; laftly, putrefaction. To give fome countenance to
this fuppofition, let fome of the effects of electricity and
fire be placed in a comparative view. Firft, a fmall iron
wire, held in the flame of a candle till it acquires a white
heat, will frequently burft into little balls, flying off in
all directions. The fame effect is produced by a flint and
fteel; and in a fuperior manner, by a ftrong charge of
electricity, or a flafh of lightning paffing through fuch

a fmall

a fmall wire; the balls then appearing, on examination, to be little more than the *fcoriæ* of the metal. The effect of electricity, lightning, and fire, in deftroying the power of the artificial or natural magnets, is a circumftance that hath been often remarked, and repeatedly publifhed. The effects of electricity, in common with fire, on proof-fpirit, gun-powder, *phofphorus*, dry lint, and many other fubftances, muft occur to every gentleman converfant in thefe experiments; indeed the parallel might be continued much further. But it may be afked, if this be really the fact, fhould not metals become electrical by friction? I anfwer, they are readily excited, provided they be firft properly infulated; (but if metal be rubbed againft metal, the phlogifton or latent fire, if I may be allowed the expreffion, is fo nearly proportioned in the two metals, that the equilibrium is reftored as foon as deftroyed, from the very nature of the bafe, which is the moft perfect conductor we are acquainted with) to illuftrate this, let it be remembered, that though the hydroftatic paradox may be readily explained, yet the fluid muft be confined in a proper veffel; and though the weight, the fpring, and the compreffibility of the air, be eafily demonftrable, a fuitable apparatus muft neceffarily be employed for each purpofe.

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It is a queſtion by no means decided, how the clouds become electrified? But if we ſuppoſe the electric matter to be a pure, ethereal, elementary fire, reſident in all bodies; that the great proceſs of vegetation is carried on by means of this ſubtile, active, volatile, and pervading element; that it is continually exhaling from all the vegetable tribe; that as evaporation is a remarkable agent in the cooling of heated ſubſtances, that is, a good conductor of their fire, as I am well aſſured it is of electricity; may we not conclude, that this is one great cauſe of the clouds becoming at times ſurcharged with this fluid? The great effect of electricity in promoting vegetation, hath been fully proved by Dr. DE MAIMBRAY, the abbé NOLLET, Mr. JALLABERT, and other gentlemen, and was very remarkable in that year when the fatal earthquake happened at Liſbon. Dr. STUKELEY's obſervations on the frequent appearances of fire-balls, coruſcations, and *auroræ boreales*, at this time (which I well remember) deſerve to be particularly noticed; and it is generally remarked, that thunder-ſtorms are preceded by a continuance of hot weather, and that a moderate temperature immediately ſucceeds the ſtorm. The remarks and obſervations of the worthy Dr. HALES on this ſubject ſeem alſo to merit peculiar attention. Further, as the rays of the ſun, concentrated by a powerful burn-

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ing mirror, will produce a fufion of metals, and inftantly reduce a number of fubftances prefented to the focus to a *calx*, as the fame effect is in many cafes produced by a ftroke of lightning; and as the colours of the electric and folar light are equally divifible by the prifm; may not thefe alfo bear fome kind of relation to each other[1]? Upon the whole, is there not an high degree of probability in the fuppofition, that light, fire, phlogifton, and electricity, are only different modifications of one and the fame principle? See notes 7. 8. and 9. A fimilarity in feveral of the phenomena of electricity and magnetifm hath been long fince pointed out by Dr. PRICE, from M. ÆPINAS; and the effect of heat on both admirably difplayed by Mr. CANTON. Of all the fubftances I have yet examined, the moft difficult to excite, I obferved to be a fine, fmooth, unarmed load-ftone, and a piece of black lead; thefe feemed to bid defiance to all my rubbers: at length, however, with a piece of new flannel they were both excited, in a very fmall degree, negatively. In fhort, I have not yet met with a fingle article (on which the experiment could be tried) that I could not, with one or other of my rubbers, make in

[1] Many other particulars might be adduced in this place; but they are purpofely omitted, this paper being already extended far beyond the limits originally intended by the author.

fome degree electrical.   The laws by which all thefe
fluids are governed, and what conftitutes the precife dif-
ference between them, may yet, perhaps, by fome fortu-
nate philofopher, by a train of juft reflexion, and a fet of
happily contrived and well-conducted experiments, be
much farther elucidated.   Laftly, I do not fpeak of thefe
things as facts of which I am abfolutely convinced; but
earneftly wifh to recommend them to the ferious con-
fideration of future enquirers.   From what hath been
faid, however, I apprehend it will fcarcely be doubted,
that electricity, whatever it be (as I have often remarked)
is one of the greateft and moft important agents in the
operations of Nature; that the effects of lightning, there-
fore, are but as difcords in her harmony; and, though
fingly confidered, they may appear unpleafing notes,
yet perhaps may be neceffary to fill up and compleat her
grand and general chorus.

NOTES ON THE CONCLUSION.

1. I am juft informed by Mr. ADAMS, that Mr. CLARKE,
an ingenious gentleman from Ireland, hath lately proved,
to the fatisfaction of fome of the ableft chemifts there,
that the variety of airs produced by different gentlemen

<div align="right">in</div>

in their pneumatical refearches (that produced from the *calces* of metals perhaps excepted) are only phlogiftic vapours arifing from, and partaking of, the qualities of the fubftances from which they are difengaged.

" The vapour of fermentation is much more fubtle
" than common air, it paffes through bodies which
" would be impenetrable obftacles to the latter.

" Mr. DE SMETH was not able to retain it by the aid
" of lutes: a moiftened bladder, tied over the mouth of a
" veffel which contained fome fermenting matter, was
" not at all inflated during the height of fermentation.

" Dr. PRIESTLEY has obferved, that the fixed air from
" fermenting beer combines eafily with the vapour of
" water, as alfo with the fmoke of rofin, fulphur, and
" other electrical fubftances.

" If it were permitted me to indulge in conjectures, I
" fhould fay that fome experiments induce me to be-
" lieve, that every elaftic fluid refults from the combina-
" tion of fome folid or fluid body with the inflammable
" principle, or perhaps even with the matter of pure
" fire; and that on this combination the ftate of elafti-
" city depends." See HENRY's tranflation of M. LAVOI-
SIER's Effays, phyfical and chemical.

Mr. LANE, in his curious and moft important experi-ment of diffolving iron in water impregnated with fixed

air, obferved, that after the water fo impregnated had been paffed through a clofe filtering paper, it was rendered quite tranfparent, the iron being in perfect folution. This clear liquor he endeavoured to preferve in its tranfparent ftate, by ufing every means that then occurred to him to retain the elaftic vapour, but without fuccefs, for in a few hours the tranfparency diminifhed: afterwards the liquor became opaque, and depofited the iron that had been diffolved in it.

2. Several gentlemen have obferved, that in working their electrical machines with great velocity, as heat was produced by the friction, the electricity was proportionably leffened.

3. Mr. ERRINGTON, a gentleman who often recreates himfelf with mechanical operations, frequently obferved, that after he had been for fome time brifkly working his drill, the ftring of it became ftrongly electrical.

4. Mr. CAVALLO, who fometimes amufes himfelf with the violin, having played a few fprightly airs, examined at my requeft the hairs of the bow, and the ftrings of the inftrument, and found by his electrometer that both of them were electrical; the former in a *plus*, the latter in a *minus* ftate. In this cafe, the rofin contributes to

the electricity. Perhaps a tenor or bafs-viol might produce the effect in a greater degree.

5. Mr. CAVALLO likewife informs me, that taking two pieces of broken China ware, he ftruck the edges of them brifkly together, and produced fparks of fire, but no electricity. He then rubbed the broader furfaces gently together, and produced a ftrong electricity; pofitive in one piece, negative in the other. This experiment I have feveral times repeated to my entire fatiffaction.

6. I have myfelf obferved, that two glafs tubes, rubbed brifkly together, produce a vivid purple light and ftrong phofphoreal fmell, but no attraction or repulfion; but two pieces of plate glafs, each two inches long and one inch broad, warmed and rubbed gently againft each other, produce electricity, negative in one piece, pofitive in the other. Both glafs and amber I have alfo made electrical by blowing upon them (previoufly warmed) with a pair of bellows.

7. Platina, in the pureft ftate to which it could be reduced by chemiftry, and on which Dr. LEWIS informed me that the ftrongeft fires he could raife had no further effect, I have been able to fufe in a fmall degree, by a ftrong charge of electricity. Phil. Tranf. vol. LXIV. p. 416.

8. Since

8. Since the learned and accurate F. BECCARIA pub-
lifhed the account of-his curious experiment of revivi-
fying the *calces* of metals by electricity, it hath been re-
peated with perfect fuccefs by feveral other gentlemen.

9. With refpect to earthquakes, upon this hypo-
thefis Dr. STUKELEY's and the rev. Mr. MITCHEL's inge-
nious theory may both be near to truth, as the difference
between them will confift more in words than in facts.
See a moft curious and aftonifhing effect of evaporation
produced by electricity in Dr. FRANKLIN's Experiments
and Obfervations, firft edit. p. 415. Perhaps it may not
be improper to mention in this place the following
experiment, which I made long fince myfelf.   A
pretty large wine-glafs being nearly filled with water,
two wires, terminated by fmall brafs balls, were hung
oppofite to each other upon the brim of the glafs, fo as
to let the balls defcend to about half the depth of the
water.   The communication being then compleated by a
chain, a jar containing three fquare feet of coated fur-
face, was difcharged through it.   The confequence was,
the ftem of the glafs was broken in two places; the bowl
was fhivered perhaps into a thoufand pieces, and fcat-
tered with the water in all directions: part of it flew into
my face, and fo much upon the apparatus, that I remem-
ber it put an end to my experiments for that time. I had
neglected to cover the glafs, being defirous to fee the

effect

effect of the charge paffing through the water; not fuf-
pecting the danger of the electricity evaporating part of
it, and exploding with fuch violence as might have been
attended with very difagreeable confequences.

10. That water-fpouts are really occafioned by elec-
tricity I have long fufpected, from feveral circumftances;
but Mr. GEORGE FORSTER, F. R. S. in his curious remarks
and circumftantial defcription of one of thefe phenomena
(Voyage round the World, vol. I. p. 191.) feems to have
confirmed this matter beyond a doubt: the form of the
column, the hail-ftones which fell at the time, and the
flafh of lightning which appeared at the disjunction of
the tube, are, I apprehend, as complete proofs as can be
given, or as the cafe can admit or require.

11. Some gentlemen have fuppofed, that the electric
matter is the caufe of the cohefion of the particles of bo-
dies. If the electric matter be as I fufpect, and my expe-
riments and the foregoing notes feem to prove, a real ele-
mentary fire inherent in all bodies, that opinion may
probably be well-founded; and perhaps the foldering of
metals and the cementation of iron by fire may be confi-
dered as ftrong proofs of the truth of their hypothefis.

12. Dr. PRIESTLEY obferves (Experiments and Obfer-
vations on Air, vol. I. p. 280.) That it is probable, that elec-

tric

tric light comes from the electric matter itself; that this being a modification of phlogiston, it is probable that *all light* is a modification of phlogiston also; and that, prior to his deductions from electrical phenomena, it was pretty evident that light and phlogiston are the same thing in different forms or states. Dr. PRIESTLEY's third volume on the same subject was not published till the last sheet of my paper had been composed, and a proof struck off. In the appendix to that volume I find so curious an article in a letter from signor VOLTA to the Doctor, that I shall take the liberty to transcribe a part of it, as a very important addition; *viz.* " I fire inflammable air
" by the simple electric spark, even when the electricity
" is very moderate, which explains the *ignes fatui*, pro-
" vided they consist of inflammable air issuing from
" marshy ground by the help of the electricity of fogs
" and by falling-stars, which are very probably thought
" to have an electrical origin.

" I do not know whether you have ever tried the effect
" of the Bolognian phosphorus on air. It phlogisticates it
" in the highest degree, and the diminution it occasions
" takes place very quickly, and is altogether surprising;
" but for this purpose the phosphorus must be good,
" and the weather not too cold."

Since
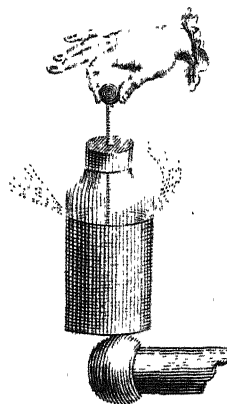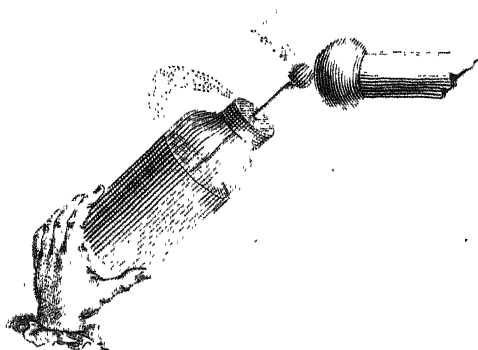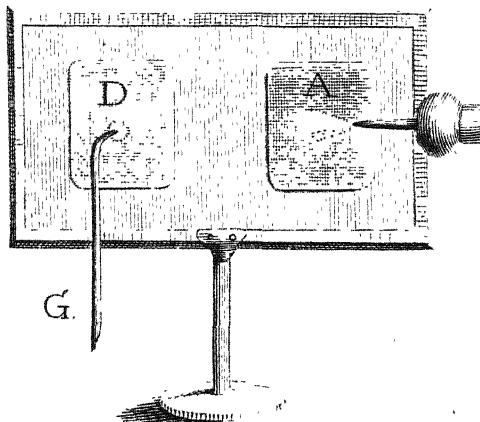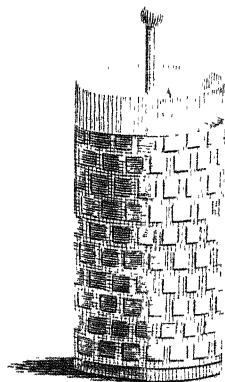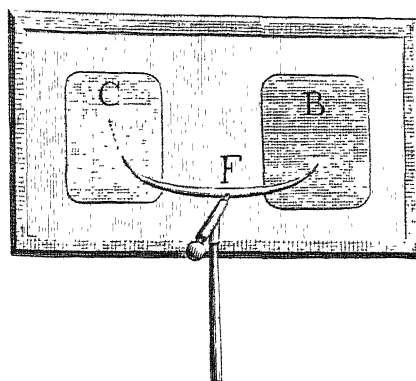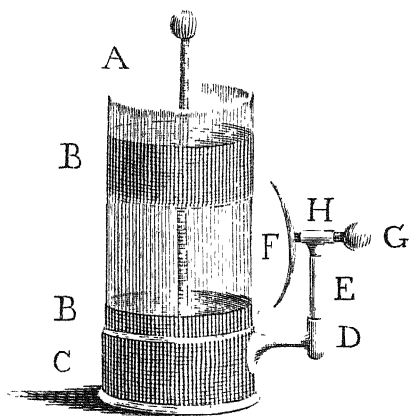
Since the preceding papers were preſented to the Royal Society, I have ſeen and heard of ſuch a number of curious remarks, obſervations, and diſcoveries on light, fire, phlogiſton, and electricity, which tend to illuſtrate and confirm the opinions I have advanced, that I would beg leave to add, that, had I ſeen or known of ſeveral of thoſe excellent pieces in time, I ſhould certainly have availed myſelf of ſuch important labours, and have ſpoken of the ſubjects above-mentioned with a greater degree of confidence. The authors I allude to are, BOERHAAVE on Fire; STAHL on Phlogiſton; Dr. PEMBERTON on Fire; Dr. HIGGINS on Light; the celebrated MACQUER, particularly in his Memoir on Phlogiſton, in the abbé ROZIER's Journal for Nov. 1776; a Memoir by Mr. OPOIX on Phlogiſton and Light; Mr. ACHARD's Electrical Experiments on the Ice of diſtilled Water, frozen in a degree of cold exceeding what we ever experience in this country; with plates of which ice he not only performed the Leyden experiment, but even excited it by friction like glaſs (ſee the Abbé ROZIER's Journal for Nov. 1776); and laſtly, M. KOESTLIN's curious and moſt valuable experiments on the influence of electricity, in the production and ſupport of animal and vegetable life, particularly his diſcovery that vegetation was actually retarded by electrifying his ſeeds negatively

VIII. *Extract of a Letter from* John Strange, *Esquire, His Majesty's Resident at* Venice, *to Sir* John Pringle, Bart. P. R. S.: *with a Letter to Mr.* Strange *from the Abbé* Joseph Toaldo, *Professor in the University of* Padua, &c. *giving an Account of the Tides in the Adriatic.*

S I R,   Venice,
                                                        Nov. 19, 1776.

Read Jan. 23. and Feb. 6. 1777. IN your last favour you expressed a desire of having some account of the the course of the tides here. As I have hitherto had but little opportunity of making any conclusive observations on that subject, I applied to the Abbé TOALDO, professor of astronomy and meteorology in the university of Padua, as the most likely person to inform me. He obligingly complied with my request by the enclosed letter, which I hope will be acceptable to you, and in some measure satisfactory, being chiefly grounded on the observations of a very accurate man here, signor TEMANZA, a celebrated architect and engineer. It is a pity but those observations had been extended to a longer term; but as no better are to be had at present, we must be contented, and hope for better hereafter. In the mean time it will be a further satisfaction to the learned professor, as well

as

as to me, SIR, should this communication prove also acceptable to the gentlemen of the Royal Society, to whom you may probably think proper to present it.

De reciproco Æstu Maris Veneti. Ad Nobilem ac Doctiffimum *Joannem Strange*, &c. Epiftola *Jofephi Toaldi*, &c.

QUÆRENTI tibi, vir præstantiffime, notitiam reciproci maris æstûs, in portu atque æstuario Veneto obtinentis, idque nomine Regiæ Societatis vestræ, clariffimique ejus præsidis D. JOHANNIS PRINGLE, baronetti, libentiffime, pro modo ac viribus, morem gero. Quî enim negare poffem vel tibi, studiorum meorum fautori munifico, rerumque naturalium fcrutatori eximio? vel Regiæ Societati, cujus acta cognitionum humanarum rerum gazophylacium, in publicum usum paratum, exhibent? vel illustri ejus præsidi, ob fcientiarum amorem, in Europâ totâ tam celebrato? vel ipsi heroicæ nationi Britannicæ, pro rerum maritimarum præstantiâ, notitiam hanc præcipuo jure quasi petenti. Accedit, quod res hæc illustrium autorum falsâ traditione infecta ac turbata est, ficut mox docebo; ut proinde intersit non minus navigationis quam fcientiæ, veram ejus historiam

memoriæ

memoriæ ac litteris tradere. Conabor ergo præcipua ac fumma rei capita breviter colligere. Ipfe quidem, Patavii degens, obfervationum feriem, quod utinam licuiffet, inftituere non potui. Sed D. THOMAS TEMANZA, architectus Venetus clari nominis, diaria obfervationum fuarum liberaliter mecum jamdiu communicavit; ac, licet femel in die obfervaverit, aquarum tamen motus ac leges in æftubus reciprocis fatis indicant: et autores alios confului fatis idoneos; et ipfe identidem, Venetias digreffus, adnotare aliqua minime neglexi.

1. Primum igitur, illud etiam vulgo notum, Venetiis, nempe in portu atque æftuario Veneto, fingulis diebus, bis aquarum acceffum five fluxum fieri, bifque receffum, five refluxum; fingulis vero menfibus lunaribus acceffus fieri majores per aliquot dies circa novam plenamque lunam, quæ Veneti appellant *Punti d' acqua,* quibus tantum, navigia majora in portum compelli poffunt, vel ab eo educi.

2. Quod præcipue fcire intereft in hoc negotio eft hora, quâ æftus menftruus ad culmen pervenit, quâque portum pertranfire poffunt. Dubitare quis poffet propter JANI PLANCI, aliorumque fortaffe fcriptorum autoritatem. PLANCUS autem in *Specimine reciproci æftûs in mari fupero,* recufo Romæ A. D. 1760, una cum libro *de conchis minus notis,* haud hæfitanter tradit;

æftum

æftum maris fequi curfum potius folis quam Lunæ; id eft, aquas in quolibet acceffu maxime elatas deprehendi fole meridianum obtinente, maxime vero depreffas fole ad horizontem delato. Quid PLANCUM aliofque in errorem induxerit, pronunciare non aufim. Fortaffe caufa erroris fuit hæc: æftus majores contingunt circa novam plenam- que Lunam: iftorum autem acceffuum hora parum diftat, ut mox oftendam, a Lunæ tranfitu per meridianum; qui iis diebus parum item diftat a tranfitu ipfius folis per eun- dem circulum. Ecce ergo caufam erroris, quæ certè impo- fuit ipfi JANO PLANCO; qui, ut in præfatione libri fui fatetur, ad portum Ariminenfem, et ad motum aquarum obfervandum, non nifi tempore majorum æftuum novi- lunii ac plenilunii defcendere confueverat; circa quadra- turas, æftus modici funt, vixque fenfibiles, ut proinde ipforum horam (turbatam etiam ob divulfionem folis ac Lunæ) obfervare non tam fit promptum.

3. Quæcumque fuerit errandi caufa certe errarunt: æftus enim maris, ut alibi, ita Venetiis, regitur a motu Lunæ. Doctor VINCENTIUS MIOTTI, obfervator diligens ac navus, MURIANI, quæ urbs milliari uno circiter a Ve- netiis diftat feptentrionem verfus, anno 1766 et 67, cum tabulas quafdam huc pertinentes conficere vellet, per plures menfes æftus accedentis ac pleni horam explo- ravit, eamque, in fummâ, comperit effe unam cum di-

midiâ

midiâ ante tranfitum Lunæ per meridianum fuper vel fubter; vel potius decimam cum dimidiâ poft: dico potius poft, quia credibile eft, aquam, quæ ob æftum in Mediterraneum ex oceano influit, tantum temporis infumere ut ad extremas oras maris Adriatici perveniat. Eandem horam colligo ex obfervationibus anno 1770 hortatu meo habitis a laudato domino TEMANZA (nam in diariis præcedentibus hoc neglexerat): idem ipfe, cum Venetiis non femel effem, ftudiofe rem animadvertens atque explorans, verum effe deprehendi.

4. Hora ergo completi portûs, quem ftatum *etabliffement du port* appellant Galli, id eft æftûs altiffimi in fyzigiis præcipue, eft $10\frac{1}{12}$ poft Lunæ tranfitum per meridianum, fuperiorem, aut inferiorem, ita ut poftea defcendere incipiant. Lunâ vero ad horizontem, five orientalem five occidentalem, accedente, aquæ humiles ac depreffæ funt, ac tum demum crefcere incipiunt [a].

Ex.

(a) Hujufmodi aquarum vices in portu Veneto atque æftuario, fi autoritate opus fit, traditas deprehendo in veteri codice manufcripto, rationem univerfam Venetæ navigationis, tunc temporis continente, una cum ampliffimo portulano: confulendum olim præbuit fereniffimus MARCUS FOSCARENUS, Venetiarum prius hiftoricus, deinde dux, vir dum viveret artium ac doctorum patronus munificentiffimus; fcriptus eft codex linguâ Venetâ vernaculâ, eâque antiquiore. Opinabatur fereniffimus dux, fuiffe codicem PETRI LAURETANI, ftrenuiffimi ducis maritimi ex noftris, qui anno 1443 extrema nece Januenfes apud RAPALLUA debellavit. Is ergo codex inter præcepta navigandi ad rem noftram hæc habet, p. 51. *Le acque di quefto porto* (di Vinetia) *fi xè quefte: quando la Luna* xè

Ex hoc igitur fcriptore (in adnotatione) habemus vices æftûs, cum diurni tum menftrui, in portu Veneto. Quod vero afferit, Lunâ in meridiano exiftente aquas fieri plenas, latius eft fumendum; ut intelligatur circa illam horam, id eft cum parvâ differentiâ unius ac dimidiæ horæ, quam fuperius ftatuimus.

5. Confequitur, ut dicamus de magnitudine, five altitudine aquarum in æftu. Circa fyzygias crefcunt aquæ plerumque tres pedes, vel tres cum dimidio fere, juxta menfuram Venetam (pes autem Venetus ad Londinen-

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xè in *Ponente* e *Levante, fono tutte le acque baffe*; e quando xè in *Siroco* e *Maiftro, fon mezze piene*; e quando la *Luna* xè in *Oftro, le acque fon tutte piene*; e quando la *Luna* xè in *Greco* e *Garbin, le acque fon mezze vode.*

Avifote, che le acque in fede comenza zorni quattro della Luna infin a zorni 10; e la xè a ponta; e dai 11 infufo fono acque feconde infina a zorni 19 della Luna; e da zorni 19 infina zorni 25 fono le acque infede; e dai 25 infina quattro della Luna fona crefcenti. Avifote, che in una Luna fono do fede, e do feconde; e da 7 alli 9 l'acqua no xè move, zoè non à poffo: quæ hanc vim habent.

" Regula aquarum in portu Veneto eft hæc: cum Luna eft in occafu vel
" ortu, aquæ funt prorfus humiles; cum refpicit plagas intermedias ventorum,
" id eft cum mediam altitudinem afcendendo vel defcendendo occupat, aquæ funt
" femi-plenæ, vel femi-vacuæ. Denique cum Luna eft in auftro, vel meri
" diano, aquæ funt ubique plenæ."

Hæc de æftu diurno, quod fequitur pertinet ad æftus menftruos.

" Admoneo, aquas deficere incipere, vel æftus parvos fieri a quartâ die Lunæ
" ufque ad decimam, ac tum incipiunt crefcere; a die undecima ufque ad unde-
" vicefimam fecundæ funt, vel plenæ; a decimâ nonâ ad vicefimam quintam
" rurfus deficiunt, et deinde iterum crefcunt ufque ad quartam diem Lunæ.
" Admoneo intra Lunationem effe duas periodos, minorum, totidemque ma-
" jorum æftuum; a feptimâ autem die ad nonam (addendum etiam 22 ad 24)
" aquas minime moveri."

fem fefe habet ut 1540 ad 1351⅔) raro ad quatuor, rarius ad quinque, rariffime aut fere numquam ad fex, auftro flante, mari irato ac procellofo.

6. Circa quadraturas elatio aquarum multo minor eft, aliquando vix trium pollicum; media, ut poftea often-dam, eft pedis 1⅓.

7. Indicata altitudo æftûs, maxima eft quæ obfervetur in toto Adriatico (cumulantur enim aquæ intra Veneta æftuaria): quo enim magis acceditur ad Mediterraneum, minores continuo deprehenduntur æftus, ita ut in Mediterraneo ipfo vix animadvertantur, exceptis euripis, aliisve anguftis finubus.

8. Omitto quæ funt communia æftûs maritimi in omnibus maribus; ex. gr. retardare fingulis diebus cum Lunæ tranfitu per meridianum; ex duobus diurnis æftubus, alterum effe majorem et diuturniorem; æftus maximos circa fyzygias fere nunquam contingere ipfo die fyzigiæ, fed vel antevertere, vel retardare, uno, duobus, tribus, aliquando vel quatuor diebus.

9. Peculiaria quædam adnotabo, ut illud cum JANO PLANCO, aquas, initio acceffûs, paulatim ac lenè crefcere per tres aut quatuor horas, ut vix tres pollices horis fingulis afcendant; poftea vehementer infurgere curfu valde citato; tum fieri, in culmine, quoddam quafi *aquiftitium*, ut femihorâ quiefcere videantur, poftea velociter

descendere,

defcendere, et eafdem vices reciprocè fubire; unde patet fpatium tranfeundi portûs quatuor horis ad fummum in majoribus æftubus circumfcribi.

10. Illud præcipue peculiare videtur huic mari noftro, atque Adriatico toti, æftus æquinoctiorum (Lunâ novâ vel plenâ) effe quidem magnos, fed non totius anni maximos; fed maximos contingere circa folftitium hybernum. Sciendum præterea, finum Venetum pleniorem in genere deprehendi, et aquas altiores effe hyeme, quam æftate: animadverterat hoc JANUS ipfe PLANCUS in laudatofpecimine. Id oftendo duplici comparatione.

11. D. TEMANZA obfervationes fuas inftituit ab anno 1751 ad 1755. Ex hoc quinquennali diario excerpfi æftus medios fingulorum anni menfium, quos in duas claffes tribuo, fex hybernos, fexque æftivos.

Menfes hyberni.			Menfes æftivi.		
Æftus med. in ped.	poll.		Æftus med. in ped.	poll.	
Januarius,	2	1,9	Aprilis,	1	9,9
Februarius,	2	0,3	Maius,	1	9,5
Martius,	1	9,7	Junius,	1	11,7
October,	1	10,9	Julius,	1	9,9
November,	2	1,4	Auguftus,	1	7,9
December,	2	2,6	September,	1	9,2
Med. diurnus,	2	0,5	Med. diurnus,	1	9,7

Patet ex hâc tabellâ, 1°, Maximum æstum totius anni,
Venetiis, contingere menfe Decembri, fcilicet circa fol-
ftitium hybernum; et huic proximum effe æstum menfis
Januarii.   2°, Minimum accidere menfe Augufto.   3°,
Mediocrem effe menfibus æquinoctialibus Martio ac Sep-
tembri.   4°, Inter fex menfes æstivos, menfem folftitia-
lem Junium afferre præ cæteris æstum magnum, et qui-
dem majorem æquinoctialibus ipfis.   5°, Tandem men-
furam æstuum æstivorum valde fuperari a menfura
æstuum hybernorum.

12. D. TEMANZA, in æstu adnotando, refpexit ad
terminum quendam medium, qui Venetiis ab aquarum
architectis appellatur *commune:* intelligunt libellam
quandam mediam aquarum in lacunâ.   Quoad ergo
hanc libellam, five altitudinem mediam aquarum, adno-
tavit, quæ menfura æstûs, tam accedendo quam rece-
dendo, fuiffet fingulis diebus fupra vel fubter (accidit
enim aliquando ut æstus ad eam altitudinem mediam mi-
nime pertingat).   Contuli ego in fummas ex unâ parte
dies, quibus fuit fupra, ex aliâ, quibus fuit infra; idque
pro utrâque menfium claffe.

Æstus

Æstus mensibus hyb.			Æstus mensibus æst.		
	Dies supra med.	Dies inf. med.		Dies supra med.	Dies inf. med.
Januario,	105	51	Aprili,	82	68
Februario,	86	55	Maio,	85	80
Martio,	95	60	Junio,	73	77
Octobri,	86	69	Julio,	64	91
Novembri,	98	52	Augusto,	63	92
Decembri,	102	53	Septembri,	75	75
Summæ,	572	340		442	483

Etiam ex hâc tabellâ difcimus, æftus hybernis menfibus fieri fæpius elatiores, et finum noftrum pleniorem effe, quam æftate.

13. Phænomenon hoc ab aliâ caufâ repetere nefcio, quam a viciniâ majore folis, in fine Decembris ad perigæum delapfi; nifi forte partem aliquam fibi vindicet incitatio major, eo temporis, in annuo telluris motu; ut aliquid fortaffe concedendum fit etiam ingeniofæ GALILÆI theoriæ; ut dum aquæ, vi attrahente corporum cœleftium, attolluntur, patiantur fimul agitationem aliquam ob inæqualem motum telluris.

14. Porro fex illis menfibus hybernis contingunt etiam procellæ majores, atque inundationes urbis, in viis, foris, ac locis minus eminentibus. Hoc loco quæftionem de majore altitudine aquarum, atque elatâ fuperficie maris noftri minime movebo. Illam tractarunt abunde CL. MANFREDIUS in *Actis Academiæ Bononenfis*, tom. II.; JANUS PLANCUS, l. c.; CL. ALBERTUS FORTIS in *Defcriptione infulæ* CHERSO ATQUE OSERO. § XVI.; probaruntque, aquas Adriatici reverâ altiores effe hifce annis quam fæculis præcedentibus. Nunc fane fæpius acceffus aquarum Venetiis pertingunt ad ea loca, ad quæ nunquam antea perveniebant. Itaque neceffe eft hodie attollere ftrata viarum, ac præcipue cifternarum, quæ aquas pluvias excipiunt, ne in magnis æftubus ab aquâ falfâ inficiantur. Nec mirum fit, in lacunâ urbem ambiente, exiftere tractus non exiguos qui ab æftu fere jam nunquam regantur; folum enim lacunæ elatum eft, atque extollitur quotidie (licet magnis ac prorfus regiis fumptibus omnes fluvii a lacunâ jamdiu depulfi fuerint) ob fedimenta aquarum, ac maris alluviones.

Ad æftus quod attinet, ex diario domini TEMANZA, atque ex folutis quibufdam fchedis, erui menfuram mediam æftûs, annis hifce, prout in appofita tabellâ.

Æftus

Æstus medius annorum.

Anni	Æstus medius.		
	ped.	poll.	
1751	1	11,82	
1752	1	10,53	
1753	2	0,35	1 11,66
1754	1	11,88	
1755	1	11,71	
1760	2	1,11	
1764	2	3,32	
1765	2	0,57	2 1,26
1766, Jun.	2	0,16	
1769	2	1,13	

Si quinque posteriorum annorum observationes fideles essent, ut priores (quod tamen ne ipse quidem D. TE-MANZA omnino spondet) manifesto evincerent, æstum maris Venetiis, annis hisce proximis, summatim crescere. Quod credibiles reddit observationes ipsas est id, quod modo aiebam, aquas hoc tempore, quâcunque de causâ, insolitos accessus facere, et inundare ea loca quæ numquam antea attingebant. Satis de hâc quæstione.

15. Modo,

15. Modo, datâ occafione, non eft tacendus curfus quidam generalis aquarum in toto Adriatico, qui eft extra controverfiam, et cujus cognitio non inutilis effe poteft ratione navigationis. Detectus eft ergo curfus quidam, feu motus aquæ, qui ingreditur finum noftrum a dextrâ parte, fitque fecundum littora Epiri, Dalmatiæ, atque Hiftriæ; torquetur per finum Tergeftinum, refluitque radendo oras Forojulienfes, Venetas, Ravennates, reliquas in ditione pontificiâ ac regni Neapolitani, egrediturque a parte finifttrâ. Hinc nautæ noftri, cum a Corcyrâ atque Ionio, Venetias petunt, legere confueverunt Epiri ac Dalmatiæ littora; cum contra Venetiis Corcyram tendunt, navigare ftudent fecundum littora pontificia ac Neapolitana; contenduntque, eodem vento, ac ceteris paribus, hâc viâ multo plus itineris confici, quam fecus.

16. Circularem hunc aquarum in Adriatico fluxum detexit primus, ni fallor, inter fciptores GEMINIANUS MONTANARIUS, profeffor Patavinus, anno 1681, dum juffu publico lacunam vifitaret. Rem litteris tradidit, ut videre eft in ejus commentario Italico, cui titulus, *Il Mare Adriatico, e fua corrente efaminata*, in *Collectione Autorum qui de Aquis currentibus fcripferunt*, vol. iv. recentis editionis Florentinæ, 1768. Ex progreffu corporum aquæ innatantium (puta infularum quarumdam ex putrefactis radicibus arundinum paluftrium concretarum, quarum

3

plurimæ concrefcunt in paludibus Adrianis) arguit MON-
TANARIUS, curfum hunc aquæ in Adriatico conficere
milliaria tria vel quatuor intra horas xxiiii. At JANUS
PLANCUS, qui eundem hunc curfum et agnovit et obfer-
vatione confirmavit, ut videre eft libro laudato, putat, ex
progreffu cadaverum fluitantium, quæ aliquando curfu
hoc deferuntur ad littora inferiora Ariminenfia, putat,
inquam, effe adhuc velociorem.

17. Opinatur porro ingeniofe MONTANARIUS, fluxum
hunc in Adriatico derivationem effe curfûs generalis in
Mediterraneo. Tefte enim FOURNERIO in *Hydrographiâ*,
oceani aquæ, per fretum Gaditanum, Mediterraneum in-
grediuntur a parte dexterâ, vel Africæ; hujus littora ra-
dunt ufque ad Ægyptum et Syriam; inde convertuntur
per oras Afiæ, fortaffe circumeunt Ægeum, legunt oras
Peloponnefi, ingrediuntur in Adriaticum eo modo quo
diximus, atque inde exeundo, peragrant littora infera
Italiæ, Liguriæ, Galliæ, Hifpaniæ, ac tandem per fretum
Gaditanum, a parte finiftrâ, in oceanum exonerantur ac
revertuntur.

18. Hæc habebam quæ de motu aquarum in finu Ve-
neto proferrem. Non ingratum, opinor, erit accipere
conclufa quædam ex obfervationibus ad æftus maritimi
theoriam fimul pertinentia, ac doctrinam NEWTONIANAM
mirifice illuftrantia.

19. Conferre

19. Conferre primum placuit æstus fyzigiarum cum æstibus quadraturarum, eductis numeris mediis æstuum quinque dierum circa fingulas, elicitifque mediis mediorum fingulis illis quinque annis exactis. Et quia Luna, cum fita eft in perigæo, ob viciniam majorem, attollere magis debet aquas, quam in apogæo, hi quoque numeri additi funt: ecce tabellam.

Æftus medius ratione fitus Lunæ.

	In Novil.	Pr. Quad	Plenil.	Ult. Quad.	Perigæo.	Apogæo.
1751	2 4,5	1 5,4	2 3,1	1 6,4	2 1,5	1 10,0
1752	2 3,6	1 4,2	2 5,8	1 3,5	1 8,5	1 7,1
1753	2 7,6	1 5,9	2 5,8	1 4,2	2 4,1	2 0,6
1754	2 4,2	1 1,3	2 3,8	1 4,1	2 5,5	2 1,5
1755	2 0,6	1 5,8	2 4,0	1 4,1	2 0,9	1 10,8
Med. 5 ann.	2 4,1	1 4,1	2 4,5	1 4,5	2 1,2	1 10,1

Patet, 1°, æstus fizigiarum medios valde excedere æstus quadraturarum; ut fi numeri utriufque fyzigiæ, et quadraturæ, componantur, æftus medius fyzigiarum fit pollicum 28,3, quadraturarum 16,3, in ratione fere 7 : 4. 2°, Animadverti poteft, æftum plenilunii plerumque effe paulo majorem æftu novilunii, ut quodammodo magis videatur retrahi a luminaribus maffa globi terreftris,

4 quam

quam aquæ oceani, utroque in fitu, in novilunio quidem fecundum elationem aquarum; in plenilunio vero luminaribus divulfis, et in oppofita trahentibus, contra nifum aquarum. Hinc enim fequitur, æftum plenilunii aliquantulo elatiorem effe debere. Utcumque fit, certe 3° æftus Lunæ perigeæ, prout ratio poftulat, fuperat æftum Lunæ apogeæ, in ratione 25,2 ad 22, vel circiter 1 ad 7; quæ eft proportio apparentis diametri Lunaris a perigæo in apogæum.

20. Porro juxta theoriam phyficam, ratione fitûs Lunæ et locorum terreftrium, fi cætera fint paria, maximi fieri deberent æftus, fpectatâ tellure totâ, cum Luna imminet lineæ æquinoctiali; ratione vero loci peculiaris, aquæ magis attolli debent, cum Luna, declinatione, cognomini latitudini geographicæ ejufdem loci potitur; minime in oppofitâ. Confeci ergo fummas æftuum, pro diebus quibus Luna morata eft (quinquennio hoc) in fingulis zodiaci fignis: ex his eductos numeros medios expofui in adjectâ tabellâ.

Tabula.

Tabula aestus maris, fecundum XII. figna zodiaci, quatenus refertur ad Lunam.

	Aries.	Taur.	Gem.	Canc.	Leo.	Virgo.	Libra.	Scorp.	Sagitt.	Capric.	Aquar.	Pisces.	Æstus Annuus	Locus perigæi Lun.
1751	2 0,67	2 2,24	2 1,41	1 11,81	1 10,25	1 11,86	1 0,50	1 10,16	1 10,93	2 8,90	2 0,41	1 0,67	1 11,82	Taur. Gem.
1752	1 1,07	1 10,72	1 10,85	1 11,82	1 11,62	1 10,80	1 11,75	1 9,98	1 8,77	1 7,17	1 9,99	1 10,36	1 10,57	Gem. Canc.
1753	2 0,00	1 11,54	1 11,00	1 10,46	2 3,18	2 3,39	1 1,46	1 11,23	1 11,30	1 10,22	1 0,91	1 11,86	2 0,35	Canc. Leo.
1754	1 9,51	1 9,41	1 11,48	1 0,73	1 1,33	2 3,92	2 2,78	1 11,18	1 10,14	1 11,32	1 10,42	2 0,35	1 11,19	Virg. Lib.
1755	1 9,09	1 10,59	1 6,77	1 10,62	1 10,20	1 11,10	1 7,20	2 1,37	2 9,50	1 10,14	1 10,03	1 8,87	1 11,71	Lib. Scorp.
Med.	1 11,27	1 11,30	1 10,70	1 11,49	2 0,12	1 1,03	2 0,14	1 11,04	1 10,73	1 9,55	1 11,15	1 11,20	11,67	

Optandum fane effet, præfto effe obfervationes plurium annorum quam quinque, ut integræ revolutionis nodorum Lunæ, vel faltem apfidum; et bis fuiffe peractas diebus fingulis, pro utroque acceffu ac receffu æftûs. Interim tabula hæc oftendit: 1°, Minimum æftum in mari noftro contingere, prout jubet theoria, Lunâ fignum Capricorni obeunte. 2°, Summatim fignorum auftralium æftus minores effe, quam borealium; 3°, æftum Cancri, et auftralium omnium (exceptâ librâ) et borealium afcendentium effe maximum; fed 4°, fuperari ab æftu trium fignorum defcendentium Leonis, Virginis, ac Libræ, quod videtur principiis noftris repugnare, fed res facile explicatur. Primum enim actio Lunæ in oceanum generatim

maxima

maxima effe debet cum verfatur prope æquatorem; deinde in fignis defcendentibus, prope æquinoctium libræ, collectio quædam cumulufque aquarum fieri debet, ex præcedente impreffione, in fignis borealibus (ratione climatis noftri) et ob vim inertiæ in aquis ipfis. Quare æftus circa Virginem ac Libram debent effe generatim maximi, prout funt in tabulâ.

21. 5°, Adnotavi pro fingulis annis locum perigæi Lunaris; ex quo apparet, perigæum ipfum, motu fuo, altiores æftus fecum quafi trahere a figno in fignum, a Tauro ac Geminis, 1751; ad Cancrum, 1752; ad Leonem, 1753; ad Virginem ac Libram, 1754; ad Scorpionem, 1755. Præterea anno 1753, quo anno perigæum proximum erat vertici noftro, æftum annuum videmus fuiffe maximum: omnia juxta theoriam.

22. Similes vices, hortante CL. LAMBERTO, academico Berolinenfi, ex obfervationibus quadraginta annorum CL. POLENI, R. S. S. quæ funt apud me, deprehendi in motu barometri. Sed de hoc egi in *Specimine* meo *Meteorologico*, et in opufculo, cui titulus *Novæ Tabulæ Barometri*, agamque fortaffe alias.

Interim opto, hafce qualefcumque notitias meas de æftu maris Veneti, a Societate Regiâ non inutiles judicari. Vale, vir præftantiffime, ac favere perge addictiffimo cultori tuo.

Dabam Patavii, 9 calendas Nov. 1776.

X. *A Letter from Mr.* Peter Wargentin, *F. R. S. Secretary to the Royal Academy of Sciences at* Stockholm, *to the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal; concerning the Difference of Longitude of the Royal Obſervatories at* Paris *and* Greenwich, *reſulting from the Eclipſes of* Jupiter's *firſt Satellites, obſerved during the laſt Ten Years: to which is added, a Comparative Table of the correſponding Obſervations of the Firſt Satellite, made in the principal Obſervatories.*

Vir plurimum reverende atque celeberrime,

Read Feb. 6,
1777. PETIISTI in utriſque litteris tuis, " velim obſervationes ſatellitum Jovis, " præcipuè primi, a te GRENOVICI, et a Cl. D. MESSIER " PARISIIS, ab anno 1765 habitas, inter ſe conferre, ut " inde eliciatur vera differentia utriuſque meridiani:" huic deſiderio tuo lubens ſatisfacio.

Tuis obſervationibus primi ſatellitis non niſi 17 correſpondentes MESSIERII obtigerunt; quarum 8 fuerunt

immerſiones

immerfiones, et 9 emerfiones. Ipfas obfervationes, inter fe et cum multis aliis, atque cum poftremâ editione mearum tabularum (eâ fcilicet, quam alteri editioni aftronomiæ fuæ inferuit Cl. DELALANDIUS) comparatas, videas in fubjunctâ appendice, in quâ Paris Cl. indicat obfervationes MESSIERII, in palatio Clugny; Paris O. autem, in ipfo obfervatorio regio habitas. Hîc fufficiat attuliffe refultantes a quovis immerfionum et emerfionum correfpondentium pari, meridianorum differentias.

Ex immerfionibus correfp.

	′	″
1765, Dec. 24. prodit diff. merid.	8	35
1769, Apr. 12.	9	31
Apr. 28.	9	58
1772, Jun. 9.	9	59
Jul. 11.	9	23
1774, Sept. 26.	9	49
Oct. 3.	9	26
Oct. 21.	9	47
Media ex imm. merid. differ.	9	39
Med. exclufa prima,	9	42

Ex emerfionibus.

	′	″
1767, Apr. 9. differ. merid.	9	49
1768, Mai. 11.	9	29
Jun. 3.	9	33
Jun. 19.	10	16
1769, Mai. 16.	9	26
Jun. 8.	9	45
1770, Aug. 5.	9	11
1773, Oct. 25.	9	23
Nov. 1.	9	39
Media ex emerfionibus,	9	37
Med. exclufa quarta,	9	32

Tu, vir celeberrime, fere femper ufus es telefcopio NEWTONIANO 6 pedum: CL. MESSIER plerumque GREGORIANO $2\frac{1}{2}$ pedum, vel acromatico $3\frac{1}{2}$ pedum, vel aliis

　　　　　　　æquales

æqualis fere potentiæ, quorum neutrum non plufquam centies objectorum diametros amplificat. Æquales ferè inventæ per immerfiones et emerfiones correfpondentes meridianorum differentiæ, fatis indicare videntur, ferme æqualis quoque præftantiæ fuiffe utriufque obfervatoris inftrumenta.   Alium et certiorem modum ea comparandi nefcio, præcipuè cum tu [c] nullibi indicaveris potentiam tui NEWTONIANI: nam longitudo fola ambiguum eft argumentum.

Per medium itaque deductum ex 7 paribus immerfionum et 8 emerfionum, invenitur differentia inter meridianum GRENOVICENSEM et obfervatorium CL. MESSIER, 9′ 38″; vel feclufis uno immerfionum uno emerfionum pari, utpote quæ nimium a reliquis recedentem, indicant meridianorum differentiam, 9′ 37″; adeoque foret inter obfervatorium GRENOVICENSE et regium Parifinum 9′ 35″, hoc eft 19 fecundis major quam hucufque putavimus.   Obftupui videns tantam incertitudinem circa veram meridianorum differentiam inter duo præcipua orbis obfervatoria, eaque vicina, in quibus ingens obfervationum numerus, annis

(*a*) The diameter of the aperture of this telefcope is 9.4 inches, as is mentioned in the preface to my Greenwich Obfervations, from 1765 to 1774, publifhed this year.   N. M.

plufquam

plufquam 100, habitus eft. Quid tum de aliis fentiendum? Ab unâ parte vix perfuadere mihi poffum, tantum errorem tamdiu potuiffe latere; vel comparatas obfervationes eclipfium Solis, Lunæ, occultationum fixarum, &c. tam multum potuiffe fallere: ab alterâ, egregius 15 bonarum obfervationum confenfus, haud facile fortuitus, fidem quandam mereri videbatur; nifi obfervationes fatellitum, ad determinandas accuratè longitudinum differentias, prorfus inepats judicaveris.

Ad tollendum hoc dubium, confului obfervationes primi fatellitis, eodem tempore, in ipfis obfervatoriis regiis, quarum non nifi duæ funt immerfiones, fed novem emerfiones.

Immerf.			Emerf.		
	′	″		′	″
1767, Jan. 12. diff. merid.	9	16	1766, Apr. 11.	9	55
1772, Jul. 11.	9	4	1767, Mart. 22.	9	36
Medium ex hifce duabus,	9	10	Apr. 16.	9	32
			Apr. 30.	9	5
			Mai. 9.	9	47
			Jun. 1.	10	28
			Jun. 3.	9	4
			1769, Jun. 8.	10	13
			1773, Oct. 5.	9	37
			Medium ex omnibus,	9	42
			exclufis 6â et 8â,	9	32

4

Harum

Harum longe minor eft confenfus: fi tamen fumatur medium, erit id 9′ 26″ vel 9′ 21″.

Ulterius comparavi feptem paria immerfionum et 4 emerfionum, annis 1761—1764, in utroque obfervatorio captarum: medium ex prioribus reperi 9′ 26″, ex pofterioribus 9′ 30″. Et ne quid deeffet, excuffi quoque obfervationes ante annum 1700 factas; inter quas 12 immerfiones correfpondentes indicant, per medium, differentiam meridianorum 9′ 57″; fed 7 emerfiones tantummodo 8′ 45″: ex hoc utroque medio refultat novum 9′ 21″.

Porro tentavi, mediantibus meis obfervationibus, quorum multæ tuis funt correfpondentes, multæ Parifienfibus, quæfitam ftabilire meridianorum differentiam. Ego femper ufus fum tubo achromatico DOLLONDIANO 10 pedum, cum oculari, quod objecta 90 amplificat, et valde diftincta reddit.

Immerf. correfp.				Emerf.			
	♄	′	″		♄	′	″
1765, Dec. 1.	1	12	34	1766, Mart. 5.	1	12	11
(b) 1766, Oct. 10.	1	12	14	1767, Jun. 1.	1	12	7
1768, Apr. 2.	1	13	10	1768, Jun. 12.	1	12	16
1769, Mart. 29.	1	11	47	1769, Mai. 16.	1	11	32
1774, Sept. 12.	1	12	32	Jun. 8.	1	12	19
Oct. 3.	1	12	12	1771, Aug. 17.	1	12	0
1775, Oct. 1.	1	12	59	Sept. 25.	1	12	8
				1772, Sept. 27.	1	12	14
Medium,	1	12	14	1773, Oct. 25.	1	12	31
Sed tua obfervatio, die 10 Oct. 1766,				1774, Dec. 29.	1	12	23
vix recte fe habere poteft. Illà				1775, Feb. 22.	1	12	16
neglectâ, manet ex reliquis me-				Dec. 27.	1	12	11
dium,	1	12	32				
				Medium ex his,	1	12	11

Obfervationes emerfionum fatis pulchrè confpirant: medium ex immerfionibus et emerfionibus innuit differentiam meridianorum Grenovicenfis et Stockholmenfis 1^h $12'$ $21''$. Perfuafus fum eam 1^h $12'$ $20''$ vix effe minorem. Per medium ex 8 obfervationibus correfponden-

(b) There was a miftake of twelve hours in fetting down this obfervation at Greenwich, by the clock keeping fidereal time, which made an error of $1'$ $49''$ in the reduction to apparent time. The correct time of immerfion is 16^h $59'$ $31''$, which happens to agree exactly with Mr. WARGENTIN's calculation, and the difference of longitude of Stockholm and Greenwich by this obfervation is 1^h $12'$ $14''$ inftead of 1^h $10'$ 25 fet down above; and the mean difference from all the feven correfponding immerfions is 1^h $12'$ $30''$. See the errata printed with my Obfervations. N. M.

tibus,

tibus, quæ habitæ funt annis 1761—1764, emergit 1^h 12′ 25″.

Quod attinet differentiam meridianorum obfervatorii Parifienfis et Stockholmenfis, ex 8 immerfionibus et 13 emerfionibus primi fatellitis fimul in utroque notatis ante annum 1760, conclufi eam, per medium, effe 1^h 2′ 51″. Sed 7 immerfiones et 12 emerfiones correfpondentes, factæ poft annum 1760, eam paulo minorem, fcilicet 2′ 47″, reddunt. Nihilominus, cum per obfervationes quarundam eclipfium folarium evincere conati funt celeberrimi viri, PINGRÉ, DU SEJOURS, et LEXELL, eam paucis fecundis effe majorem, non refragabor affumere eam 1^h 2′ 55″, quâ fubtractâ ab inventâ differentiâ obfervatororum Grenovicenfis et Stockholmenfis 1^h 12′ 21″, reftat differentia Grenovicenfis et Parifienfis 9′ 26″.

Omnes itaque hæ difquifitiones teftantur, differentiam quæfitam majorem effe 9′ 16″ quantam hucufque exiftimavimus; et ni fallor ad 9′ 25″ proximè accedere; de quâ re tamen totum ad te, vir celeberrime, defero judicium.

Interim hinc apparet, arduum fane effe, præcifas meridianorum differentias, ope obfervationum fatellitum Jovis, determinare. Fixarum occultationes a Lunâ, probè obfervatæ et excuffæ, certiorem fine dubio fuppeditant methodum.

<div align="right">Valde</div>

Valde miror, quare illi, qui bonum telescopium vel tubum habent, eo non semper in observandis satellitibus utantur, sed jam hoc, jam alio, forte minus præstanti. Observationes multum dubias, vel aëris vitio vel aliam ob caussam, ne quidem in diarium refero, nam nulla observatio præstat malæ. Quantum malæ observationes, pro bonis venditatæ, me confuderint et torserint, dicere non possum.

Quartus, die 8 Februarii hujus anni, eclipsin tantum partialem passus est. Magnum tuæ in me amicitiæ signum id interpretabor, si, quovis anno finito, tuas mecum communicare velis observationes satellitum; nam his, etiam senex, delector. Hanc quoque provinciam, examinandi motus satellitum, mihi, quasi tacito consensu, detulisse videntur collegæ astronomi. Ceterum ingruens ætas, nimiæ occupationes quas secretarii munus injungit, defectus instrumentorum (nam murali nondum instructus sum, quidquid dicat Celeb. LALANDIUS) ut taceam modicas ingenii vires, vetant quo minus aliquid tuâ vel aliorum exspectatione dignum præstare possim [b]. Frigus hâc hieme in Sueciâ continuum fuit, a 28 Dec. ad 5 Februarii; sed neutiquam, pro ratione climatis, præter

(c) Notwithstanding the author's modesty, the astronomers of Europe know him better, and lament with him that he is not so well provided with capital instruments as he wishes and deserves to be. N. M.

modum, rigidum: plerumque 5 vel 10 gradum thermometri REAUMURIANI. Diebus tantum 26 et 27 Januarii, ad 17 et 18 gradus exafperatum fuit. Minor quoque nivis copia apud nos fuit, quam pro folito. Miror itaque intenfitatem frigoris, eodem tempore, apud exteros.

Dab. Stockholmiæ, die 19 Mart. 1776.

Obfervationes primi fatellitis Jovis in præcipuis obfervatoriis habitæ, inter fe et cum tabulis comparatæ.

Ann. M. D.	Temp.	Obfervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obfervatorium.
1765. Sept. 21.		16 53 22	Im.	16 53 15	0 7 —	Stockholm.
Oct. 23.		13 31 6	—	13 31 30	0 24 +	Tyrnav.
Dec. 1.		10 40 11	—	10 40 48	0 37 +	Greenwich.
		11 33 25	—	11 33 33	0 8 +	Lund.
		11 50 56	—	11 51 0	0 4 +	Tyrnav.
		11 52 45	—	11 52 59	0 14 +	Stockholm.
	8.	12 31 34	—	12 31 25	0 9 —	Greenw.
	15.	14 22 3	—	14 21 40	0 23 —	Greenw.
		15 31 40	—	15 31 52	0 12 +	Tyrnav.
	17.	10 1 15	—	10 1 26	0 11 +	Stockh.
	22.	16 12 19	—	16 11 47	0 32 —	Greenw.
	24.	10 39 27	—	10 39 19	0 8 —	Greenw.
		10 48 2	—	10 48 37	0 35 +	Paris Cl. *d.*
		11 49 6	—	11 49 31	0 25 +	Tyrnav.
1766. Jan. 2.		8 8 44	—	8 9 24	0 40 +	Stockh. *d.*
	9.	9 40 16	—	9 40 37	0 21 +	Lund.
	16.	11 50 51	—	11 51 17	0 26 +	Stockh.
	23.	13 40 32	—	13 41 28	0 56 +	Upfala *d.*
	25.	8 8 30	—	8 9 30	1 0 +	Upfala *d.*

Obfervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.				Calculus.			Diff. Calc. Obfervatorium.		
	M. D.	h	′	″		h	′	″	′	″	
1766.	Jan. 25.	8	10	39	Im.	8	11	10	0	31 +	Stockholm.
	Febr. 15.	16	6	31	Em.	16	6	14	0	17 —	Upfala.
	24.	12	31	20	—	12	31	48	0	28 +	Stockholm.
	Mart. 5.	7	43	55	—	7	44	25	0	30 +	Greenw.
		8	55	4	—	8	54	56	0	8 —	Upfala.
		8	56	6	—	8	56	36	0	30 +	Stockh.
		9	45	36	—	9	45	39	0	3 +	Peterfb.
	10.	15	20	27	—	15	20	58	0	31 +	Paris. Cl.
		15	20	35	—	15	20	56	0	21 +	Paris. O.
	12.	9	49	54	—	9	50	3	0	9 +	Paris. O.
		9	49	56	—	9	50	5	0	9 +	Paris. Cl.
		10	33	12	—	10	33	32	0	20 +	Lund.
		10	50	41	—	10	50	59	0	18 +	Tyrnav.
		10	50	53	—	10	51	18	0	25 +	Upfala.
		10	52	13	—	10	52	58	0	45 +	Stockh.
		11	41	56	—	11	42	1	0	5 +	Peterfb.
	19.	12	30	15	—	12	30	8	0	7 —	Lund.
		12	47	52	—	12	47	54	0	2 +	Upfal. *d.*
	21.	7	16	36	—	7	16	48	0	12 +	Tyrnav.
	26.	13	33	36	—	13	34	13	0	37 +	Greenw.
	28.	8	12	33	—	8	12	44	0	11 +	Paris. Cl.
		9	8	56	—	9	8	53	0	3 —	Wien.
	Apr. 4.	11	10	7	—	11	10	48	0	41 +	Upfala.
		11	10	21	—	11	10	29	0	8 +	Tyrnav.
		11	12	4	—	11	12	28	0	24 +	Stockh.
	11.	11	56	30	—	11	56	59	0	29 +	Greenw.
		12	6	25	—	12	6	15	0	10 —	Paris. O.
		13	2	32	—	13	2	26	0	6 —	Wien.
	20.	8	31	18	—	8	31	49	0	31 +	Paris. Cl.
		8	31	37	—	8	31	47	0	10 +	Paris. O.
		9	27	56	—	9	27	58	0	2 +	Wien.
		9	32	30	—	9	32	43	0	13 +	Tyrnav.

Obfervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Anni.	Temp. Obſervationis. M. D.	h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1766.	Apr. 20.	9 34 24	Em.	9 34 42	0 18 +	Stockholm.
		10 23 9	—	10 23 45	0 36 +	Peterſb.
	27.	11 30 20	—	11 30 42	0 22 +	Stockh.
	May 13.	9 47 45	—	9 48 2	0 17 +	Tyrnav.
		9 49 24	—	9 50 1	0 37 +	Stockholm.
	20.	11 25 12	—	11 25 4	0 8 —	Lund.
	Jun. 5.	9 54 1	—	9 53 44	0 17 —	Wien.
		9 58 39	—	9 58 29	0 10 —	Tyrnav.
	Oct. 10. *(d)*	17 1 20	Im.	16 59 31	1 49 —	Greenw.
		18 11 45	—	18 11 42	0 3 —	Stockh.
	26.	16 27 59	—	16 28 13	0 14 +	Tyrnav.
	Nov. 2.	17 21 9	—	17 21 18	0 9 +	Paris. Cl.
		18 22 20	—	18 22 12	0 8 —	Tyrnav.
		18 24 19	—	18 24 11	0 8 —	Stockholm.
	18.	16 36 1	—	16 35 51	0 10 —	Tyrnav.
	25.	18 9 27	—	18 10 1	0 34 +	Lund. *d.*
		18 29 37	—	18 29 27	0 10 —	Stockh.
	Dec. 4.	13 44 46	—	13 45 9	0 23 +	Paris. O.
		13 44 53	—	13 45 11	0 18 +	Paris. Cl.
	18.	17 25 29	—	17 25 29	0 0	Paris. O.
1767.	Jan. 3.	15 24 1	—	15 23 57	0 4 —	Greenw.
	12.	11 41 41	—	11 42 15	0 34 +	Greenw.
		11 50 57	—	11 51 31	0 34 +	Paris. O.
	26.	15 34 34	—	15 34 40	0 6 +	Paris. Cl.
	Feb. 2.	17 26 54	—	17 27 20	0 26 +	Paris. Obſ.
		17 27 13	—	17 27 22	0 9 +	Paris. Cl.
	13.	9 18 13	—	9 18 44	0 31 +	Tyrnav.
	20.	11 14 44	—	11 15 14	0 30 +	Stockholm.
	27.	11 57 7	—	11 58 6	0 59 +	Greenw. *d.*
	Mart. 17.	8 12 16	Em.	8 11 32	0 44 —	Tyrnav.
	22.	14 28 48	—	14 28 40	0 8 —	Greenw.

(d) This obſervation rightly reduced to apparent time is 16ʰ 59′ 31″, which happens to agree exactly with Mr. WARGENTIN's calculation. N. M.

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis. M. D.	a ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1767,	Mar. 22.	14 38 24	Em.	14 37 56	0 28 —	Paris. O.
	31.	12 4 36	—	12 5 0	0 24 +	Upſala.
		12 4 54	—	12 4 41	0 13 —	Tyrnav.
		12 6 28	—	12 6 40	0 12 +	Stockh.
	Apr. 7.	14 1 46	—	14 1 35	0 11 —	Upſala.
		14 2 59	—	14 3 15	0 16 +	Stockh.
	9.	7 20 1	—	7 20 13	0 12 +	Greenw.
		7 29 50	—	7 29 31	0 19 —	Paris. Cl.
	14.	14 47 52	—	14 47 36	0 16 —	Greenw.
	16.	9 16 13	—	9 16 43	0 30 +	Greenw.
		9 25 45	—	9 25 59	0 14 +	Paris. O.
	23.	12 23 22	—	12 23 10	0 12 —	Tyrnav.
		12 23 42	—	12 23 29	0 13 —	Upſala.
		12 24 30	—	12 25 9	0 39 +	Stockh.
	30.	13 9 10	—	13 8 47	0 23 —	Greenw.
		13 18 15	—	13 18 3	0 12 —	Paris. O.
	Mai. 9.	9 32 26	—	9 32 58	0 32 +	Greenw.
		9 42 13	—	9 42 14	0 1 +	Paris. O.
		10 43 7	—	10 43 10	0 3 +	Tyrnav.
	16.	12 38 6	—	12 37 54	0 12 —	Tyrnav.
	30.	10 15 32	—	10 15 17	0 15 —	Philadelphia.
	Jun. 1.	9 44 1	—	9 44 20	0 19 +	Greenw.
		9 54 29	—	9 53 36	0 53 —	Paris. O. d.
		10 56 8	—	10 56 31	0 23 +	Stockh.
	8.	11 37 42	—	11 37 49	0 7 +	Greenw.
		11 46 46	—	11 47 5	0 19 +	Paris. O.
	Nov. 21.	19 10 24	Im.	19 10 35	0 11 +	Lund.
	Dec. 23.	14 43 47	—	14 43 45	0 2 —	Paris. Cl.
	30.	16 24 27	—	16 24 16	0 11 —	Greenw.
1768.	Jan. 22.	16 24 13	—	16 23 37	0 36 —	Greenw.
	31.	12 53 11	—	12 52 56	0 15 —	Paris. O.
		12 53 22	—	12 52 58	0 24 —	Paris. Cl.

Obſervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis.				Calculus.			Diff. Calc.		Obſervatorium.
M. D.	h	′	″		h	′	″	′	″	
1768. Feb. 14.	16	39	50	Im.	16	39	28	0	22 —	Paris. Cl.
	17	42	37	—	17	42	21	0	16 —	Stockh.
16.	12	8	37	—	12	9	14	0	37 +	Upſala.
	12	8	54	—	12	8	55	0	1 +	Tyrnav.
	12	11	9	—	12	10	54	0	15 —	Stockh.
	12	59	24	—	12	59	57	0	33 +	Peterſburg.
Mart. 1.	9	46	49	—	9	47	26	0	37 +	Philadelphia.
	14	57	57	—	14	57	21	0	36 —	Paris. Cl.
	15	58	20	—	15	58	15	0	5 —	Tyrnav.
3.	10	26	59	—	10	27	4	0	5 +	Tyrnav.
8.	17	54	32	—	17	54	10	0	22 —	Upſala.
10.	11	22	15	—	11	21	56	0	19 —	Paris. Cl.
	12	22	40	—	12	22	50	0	10 +	Tyrnav.
	13	13	25	—	13	13	52	0	27 +	Peterſburg.
17.	13	32	17	—	13	32	47	0	30 +	Geneve.
19.	9	39	0	—	9	38	56	0	4 —	Peterſburg.
24.	15	29	3	—	15	29	3	0	0	Geneve.
26.	9	43	3	—	9	43	16	0	13 +	Paris. O.
	9	58	1	—	9	58	6	0	5 +	Geneve.
	10	43	56	—	10	44	12	0	16 +	Tyrnav.
	11	34	52	—	11	35	14	0	22 +	Peterſburg.
Apr. 2.	11	29	33	—	11	30	25	0	52 +	Greenw. *d.*
	12	42	43	—	12	42	36	0	7 —	Stockh.
18.	12	1	37	Em.	12	1	14	0	23 —	Greenw.
25.	8	56	50	—	8	56	33	0	17 —	Philadelphia.
	13	57	19	—	13	57	10	0	9 —	Greenw.
27.	8	35	11	—	8	35	26	0	15 +	Paris. Cl.
Mai. 4.	10	31	0	—	10	30	56	0	4 —	Paris. Cl.
	11	32	18	—	11	31	50	0	28 —	Tyrnav.
	11	33	22	—	11	33	49	0	27 +	Stockh.
11.	12	16	46	—	12	16	46	0	0	Greenw.
	12	26	15	—	12	26	4	0	11 —	Paris. Cl.

Obſervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann. M D.	Temp.	Obfervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obfervatorium.
1768. Mai. 20.		8 49 54	−	8 49 29	0 25 —	Paris. Cl.
		9 52 6	−	9 52 22	0 16 +	Stockh.
	27.	10 43 14	−	10 43 44	0 30 +	Paris. Cl.
Jun. 3.		12 28 6	−	12 28 19	0 13 +	Greenw.
		12 37 39	−	12 37 37	0 2 —	Paris. Cl.
	12.	8 50 16	−	8 50 18	0 2 +	Greenw.
		10 0 23	−	10 0 49	0 26 +	Upfala.
		10 2 32	−	10 2 29	0 3 —	Stockh.
	19.	10 43 27	−	10 43 38	0 11 +	Greenw.
		10 53 43	−	10 52 56	0 47 —	Paris. Cl. *d.*
Jul. 5.		9 8 5	−	9 8 13	0 8 +	Paris. Cl.
1769. Jan. 17.		18 37 12	Im.	18 37 6	0 6 —	Stockh.
Feb. 2.		16 46 30	−	16 46 19	0 11 —	Tyrnav.
	16.	14 21 10	−	14 20 57	0 13 —	Norriton.
		14 21 51	−	14 21 49	0 2 —	Philadelphia.
	23.	16 15 1	−	16 14 59	0 2 —	Norriton.
		16 16 21	−	16 15 51	0 30 —	Philadelphia.
Mart. 20.		16 9 9	−	16 9 3	0 6 —	Paris. Cl.
	29.	12 25 7	−	12 24 22	0 45 —	Greenw.
		13 34 34	−	13 34 34	0 0	Tyrnav.
		13 36 54	−	13 36 33	0 21 —	Stockh.
Apr. 3.		14 49 25	−	14 49 49	0 24 +	Norriton.
		14 50 48	−	14 50 41	0 7 —	Philadelphia.
	5.	15 13 35	−	15 13 1	0 34 —	Lund.
		15 32 30	−	15 32 27	0 3 —	Stockh.
	10.	16 46 0	−	16 45 41	0 19 —	Norriton.
	12.	11 14 37	−	11 14 40	0 3 +	Norriton.
		11 15 49	−	11 15 32	0 17 —	Philadelphia.
		16 16 8	−	16 16 9	0 1 +	Greenw.
		16 25 39	−	16 25 27	0 12 —	Paris. Cl.
	21.	12 50 14	−	12 50 1	0 13 —	Paris. Cl.
		13 52 41	−	13 52 54	0 13 +	Stockh. *d.*

Obfervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann. Temp. M. D.	Obfervationis h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obfervatorium.
1769. Apr. 28.	14 35 17	—	14 36 7	0 50 +	Greenw.
	14 45 15	—	14 45 25	0 10 +	Paris. Cl.
30.	9 13 42	—	9 14 16	0 34 +	Paris. O.
	10 16 38	—	10 17 9	0 31 +	Stockh.
Mai. 5.	11 29 27	—	11 29 43	0 16 +	Norriton.
	11 30 28	—	11 30 35	0 7 +	Philadelphia.
12.	10 37 6	Em.	10 35 37	1 29 —	Otaheite. *d.*
16.	9 31 35	—	9 30 54	0 41 —	Greenw.
	9 41 1	—	9 40 12	0 49 —	Paris. Cl. *d.*
	10 43 7	—	10 43 5	0 2 —	Stockh.
21.	11 55 13	—	11 55 5	0 8 —	Norriton.
23.	11 34 52	—	11 34 28	0 24 —	Paris. Cl.
	12 37 42	—	12 37 21	0 21 —	Stockh.
28.	11 31 59	—	11 31 53	0 6 —	S. Jofeph.
Jun. 4.	10 45 31	—	10 46 31	1 0 +	Otaheite.
6.	7 53 58	—	7 54 8	0 10 +	S. Jofeph.
	10 11 32	—	10 11 27	0 5 —	Norriton.
8.	9 40 56	—	9 41 20	0 24 +	Greenw.
	9 50 41	—	9 50 38	0 3 —	Paris. Cl.
	9 51 9	—	9 50 36	0 33 —	Paris. O.
	10 51 45	—	10 51 51	0 6 +	Upfala.
	10 53 15	—	10 53 31	0 16 +	Stockh.
13.	7 8 16	—	7 8 28	0 12 +	Otaheite.
	12 5 1	—	12 5 1	0 0	Norriton.
15.	11 35 33	—	11 34 53	0 40 —	Greenw.
18.	14 33 36	—	14 33 37	0 1 +	Otaheite.
20.	9 1 43	—	9 1 57	0 14 +	Otaheite.
	11 40 56	—	11 41 11	0 15 +	Norriton.
22.	8 27 35	—	8 27 51	0 16 +	Philadelphia.
24.	9 6 41	—	9 7 3	0 22 +	Tyrnav.
27.	10 56 15	—	10 55 39	0 36 —	Otaheite.
29.	8 2 52	—	8 3 14	0 22 +	S. Jofeph.

Obfervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.					Calculus.			Diff. Calc.	Obfervatorium.
	M. D.	h	′	″		h	′	″	′ ″	
1769.	June 29.	10	21	55	Em.	10	21	25	0 30 —	Philadelph.
	Jul. 1.	9	50	24	—	9	50	31	0 7 +	Greenwich.
	•	11	0	59	—	11	0	43	0 16 —	Tyrnav.
	6.	7	18	16	—	7	17	56	0 20 —	Otaheite.
	13.	11	51	49	—	11	51	24	0 25 —	S. Jofeph.
	24.	10	12	28	—	10	11	41	0 47 —	Paris. Cl.
	Aug. 23.	7	15	48	—	7	15	14	0 34 —	Philadelph.
1770.	Jan. 29.	17	52	18	Im.	17	52	29	0 11 +	Tyrnav.
	Mart. 16.	17	2	47	—	17	2	16	0 31 —	Greenwich.
	25.	14	37	14	—	14	36	33	0 41 —	Tyrnav.
	Mai. 3.	13	9	36	—	13	9	33	0 3 —	Tyrnav.
	10.	14	45	45	—	14	46	20	0 35 +	Lund.
		15	5	43	—	15	5	46	0 3 +	Stockholm.
	26.	13	2	18	—	13	3	9	0 51 +	Berlin.
	Jun. 4.	9	40	27	—	9	41	18	0 51 +	Tyrnav.
	11.	12	35	22	Em.	12	34	27	0 55 —	Greenwich.
	20.	10	8	30	—	10	8	6	0 24 —	Stockholm.
	Jul. 13.	9	5	7	—	9	5	19	0 12 +	Greenwich.
		9	6	24	—	9	5	38	0 46 —	Chiflehurft.
		9	59	15	—	9	59	1	0 14 —	Berlin.
	29.	8	34	19	—	8	34	13	0 6 —	Tyrnav.
	Aug. 5.	9	19	41	—	9	19	57	0 16 +	Greenwich.
		9	20	42	—	9	20	16	0 26 —	Chiflehurft.
		9	28	52	—	9	29	12	0 20 +	Paris. Cl.
		10	13	31	—	10	13	34	0 3 +	Berlin.
		10	30	25	—	10	30	6	0 19 —	Tyrnav.
	21.	8	52	49	—	8	52	47	0 2 —	Tyrnav.
	Sept. 13.	8	15	14	—	8	14	46	0 28 +	Paris. Cl.
1771.	Mart. 28.	16	45	22	Im.	16	45	6	0 16 —	Paris. Cl.
	Apr. 13.	15	4	21	—	15	4	16	0 5 —	Paris. Cl.
	Mai. 22.	13	46	13	—	13	46	28	0 15 +	Geneve.

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis.			Calculus.	Diff. Calc.	Obſervatorium.
	M. D.	h ′ ″		h ′ ″	′ ″	
1771.	Mai. 22.	14 32 20	Im.	14 32 25	0 5 +	Tyrnav.
	Jun. 7.	11 44 57	—	11 44 53	0 4 —	Paris. Cl.
		11 59 43	—	11 59 50	0 7 +	Genève.
	14.	13 52 20	—	13 52 14	0 6 —	Geneve.
		14 22 4	—	14 21 39	0 25 —	Berlin.
	Jul. 23.	14 5 38	Em.	14 4 58	0 40 —	Greenwich.
	25.	9 44 15	—	9 43 51	0 24 —	Tyrnav.
	Aug. 1.	10 37 38	—	10 38 1	0 23 +	Paris Cl.
	10.	8 3 49	—	8 3 38	0 11 —	Tyrnav.
	17.	8 49 47	—	8 49 53	0 6 +	Greenw.
		8 51 0	—	8 50 12	0 48 —	Chiſlehurſt.
		9 14 12	—	9 14 8	0 4 —	Geneve.
		10 1 47	—	10 2 4	0 17 +	Stock.
	Sept. 2.	8 23 55	—	8 24 2	0 7 +	Tyrnav.
		8 26 14	—	8 26 1	0 13 —	Stockholm.
	9.	9 11 56	—	9 11 44	0 12 —	Greenw.
		9 12 18	—	9 12 3	0 15 —	Chiſlehurſt.
	25.	7 37 43	—	7 37 24	0 19 —	Greenw.
		8 49 51	—	8 49 35	0 16 —	Stockh.
	Oɛt. 2.	9 35 40	—	9 35 26	0 14 —	Greenw.
		9 35 56	—	9 35 45	0 11 —	Chiſleh.
	11.	6 2 33	—	6 2 39	0 6 +	Greenw.
		6 3 16	—	6 2 58	0 18 —	Chiſleh.
		7 13 21	—	7 12 47	0 34 —	Tyrnav.
	Nov. 3.	7 32 32	—	7 32 12	0 20 —	Tyrnav.
	19.	5 52 27	—	5 52 34	0 7 +	Stockh.
1772.	Mai. 12.	15 11 30	Im.	15 12 1	0 31 +	Pekin.
	Jun. 9. *	14 57 33	—	14 57 14	0 19 —	Greenw.
		15 7 32	—	15 6 32	1 0 —	Paris. Cl.
		15 28 20	—	15 27 55	0 25 —	Perinaldo.
	25.	13 9 17	—	13 18 43	0 34 —	Paris. O.

Obſervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.				Calculus.			Diff. Calc. Obfervatoriur.		
M. D.	r	′	″		h	′	″	′	″	
1772. Jun. 25.	13	40	9	Im.	13	40	8	0	1 —	Perinaldo.
	14	20	1	—	14	19	39	0	22 —	Tyrnav.
27.	15	22	54	—	15	23	10	0	16 +	Pekin.
Jul. 2.	15	32	31	—	15	32	27	0	4 —	Perinaldo.
11.	11	22	34	—	11	22	33	0	1 —	Greenw.
	11	22	34	—	11	22	52	0	18 +	Chiflehurft.
	11	31	38	—	11	31	49	0	11 +	Paris. O.
	11	31	57	—	11	31	51	0	6 —	Paris. Cl.
	12	32	25	—	12	32	45	0	20 +	Tyrnav.
18.	13	25	10	—	13	25	4	0	6 —	Paris. O.
	14	25	41	—	14	25	59	0	18 +	Tyrnav.
27.	9	38	2	—	9	38	25	0	23 +	Chiflehurft.
	9	47	28	—	9	47	24	0	4 —	Paris. Cl.
	11	39	6	—	11	39	20	0	14 +	Peterfb.
Aug. 3.	11	32	48	—	11	33	2	0	14 +	Chiflehurft.
	13	33	40	—	13	33	57	0	17 +	Peterfb.
10.	13	36	46	—	13	37	10	0	24 +	Paris. O.
	13	37	11	—	13	37	12	0	1 +	Paris. Cl.
	15	28	26	—	15	29	8	0	42 +	Peterfb.
21.	14	23	21	Em.	14	22	41	0	40 —	Pekin.
26.	14	4	22	—	14	4	18	0	4 —	Greenw.
28.	9	26	55	—	9	27	20	0	25 +	Berlin.
	9	44	5	—	9	43	52	0	13 —	Tyrnav.
30.	10	48	44	—	10	48	31	0	13 —	Pckin.
Sept. 4.	11	23	38	—	11	23	26	0	12 —	Lund.
	11	23	50	—	11	24	21	0	29 +	Berlin.
	11	41	11	—	11	40	53	0	18 —	Tyrnav.
8.	7	15	19	—	7	14	59	0	20 —	Pekin.
13.	8	9	44	—	8	9	38	0	6 —	Stockholm.
15.	9	12	7	—	9	12	29	0	22 +	Pekin.
20.	9	47	54	—	9	47	49	0	5 —	Lund.

Obfervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Anni.	Temp.	Obſervationis.				Calculus.			Diff. Calc.		Obſervatorium.
	M. D.	h	′	″		h	′	″	′	″	
1772.	Sept. 20.	10	6	56	Em.	10	7	16	0	20 +	Stockh.
	22.	11	9	47	—	11	10	6	0	19 +	Pekin.
	27.	10	52	31	—	10	52	47	0	16 +	Greenw.
		12	2	52	—	12	3	18	0	26 +	Upſala.
		12	4	45	—	12	4	58	0	13 +	Stockh.
Oct.	4.	12	50	41	—	12	50	41	0	0	Chiſlehurſt.
	6.	7	28	53	—	7	29	6	0	13 +	Paris. O.
		8	13	17	—	8	13	30	0	13 +	Berlin.
		8	30	12	—	8	30	2	0	10 —	Tyrnav.
	13.	9	16	49	—	9	17	13	0	24 +	Greenw.
		9	17	13	—	9	17	32	0	19 +	Chiſlehurſt.
		10	9	40	—	10	9	58	0	18 +	Lund.
		10	10	13	—	10	10	53	0	40 +	Berlin.
		10	27	31	—	10	27	25	0	6 —	Tyrnav.
	17.	6	1	36	—	6	1	26	0	10 —	Pekin.
	20.	11	13	59	—	11	14	11	0	12 +	Greenw.
		11	14	32	—	11	14	30	0	2 —	Chiſlehurſt.
	22.	6	36	57	—	6	36	59	0	2 +	Berlin.
		6	53	28	—	6	53	31	0	3 +	Tyrnav.
	24.	7	58	20	—	7	58	10	0	10 —	Pekin.
	29.	8	50	14	—	8	49	55	0	19 +	Tyrnav.
	31.	9	54	21	—	9	54	25	0	4 +	Pekin.
Nov.	9.	6	19	6	—	6	18	51	0	15 —	Pekin.
	14.	5	59	28	—	5	59	23	0	5 —	Greenw.
	16.	8	13	54	—	8	13	37	0	17 —	Pekin.
Dec.	2.	6	29	46	—	6	29	41	0	5 —	Pekin.
	9.	8	23	12	—	8	22	37	0	35 —	Pekin.
	23.	5	15	3	—	5	14	39	0	24 —	Lund.
		5	15	51	—	5	15	34	0	17 —	Berlin.
	25.	6	36	12	—	6	35	36	0	36 —	Pekin.
1773.	Mai. 29.	15	18	46	Im.	15	18	4	0	42 —	Perinaldo. *d.*

Obſervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.		Calculus.	Diff. Calc. Obfervatorium.
	M. D.	h ′ ″	h ′ ″	′ ″
1773.	Jun. 14.	14 10 37 Im.	14 10 45	0 8 + Tyrnav.
	16.	15 14 17 —	15 14 13	0 4 — Pekin.
	21.	15 16 43 —	15 16 58	0 15 + Geneve.
	Jul. 7.	13 35 59 —	13 36 23	0 24 + Perinaldo. *d.*
	14.	15 28 55 —	15 28 45	0 10 — Perinaldo.
	16.	11 27 8 —.	11 27 40	0 32 + Peterfburg.
	23.	11 19 22 —	11 19 42	0 20 + Greenw.
		11 50 22 —	11 50 17	0 5 — Perinaldo.
	30.	13 44 28 —	13 44 6	0 22 — Perinaldo.
		15 14 40 —	15 14 45	0 5 + Peterfburg.
	Aug. 6.	15 17 30 —	15 17 13	0 17 — Paris. Cl.
		15 17 36 —	15 17 11	0 25 — Paris. O.
		15 32 20 —.	15 32 10	0 10 — Geneve.
	8.	10 0 42 —	10 0 53	0 11 + Geneve.
		10 7 15 —	10 7 13	0 2 — Perinaldo.
		10 48 41 —	10 48 49	0 8 + Stockholm.
		11 37 37 —	11 37 52	0 15 + Peterfb.
	15.	11 41 5 —	11 40 47	0 18 — Paris. Cl.
		11 55 58 —	11 55 44	0 14 — Geneve.
		12 41 23 —	12 41 41	0 18 + Tyrnav.
		13 32 58 —	13 32 43	0 15 — Peterfburg.
	22.	13 26 14 —	13 27 3	0 49 + Greenw. *d.*
		15 28 20 —	15 28 17	0 3 — Peterfb.
	24.	9 8 24 —	9 8 18	0 6 — Stockholm.
		15 41 22 —	15 41 46	0 24 + Pekin.
	26.	10 10 31 —	10 10 40	0 9 + Pekin.
	29.	15 32 41 —	15 32 25	0 16 — Paris. Cl.
		15 47 40 —	15 47 22	0 18 — Geneve.
		15 53 49 —	15 53 42	0 7 — Perinaldo.
	31.	9 51 57 —	9 52 12	0 15 + Greenw.
		10 22 16 —	10 22 47	0 31 + Perinaldo.

Obfervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.				Calculus.			Diff. Calc.	Obfervatorium.
	M. D.	h	′	″		h	′	″	′	″
1773.	Aug. 31.	11	2	7	Im.	11	2	24	0 17 +	Tyrnav.
	Sept. 7.	12	18	35	–	12	19	11	0 36 +	Perinaldo.
	9.	14	2	56	–	14	3	27	0 31 +	Pekin.
	14.	14	9	8	–	14	9	36	0 28 +	Geneve.
		15	46	14	–	15	46	35	0 21 +	Peterfburg.
	16.	9	27	0	–	9	27	8	0 8 +	Stockholm.
	25.	12	26	7	–	12	26	27	0 20 +	Pekin.
	27.	9	6	28	Em.	9	6	30	0 2 +	Pekin.
	Oct. 2.	9	58	55	–	9	58	41	0 14 –	Tyrnav.
		10	49	49	–	10	49	43	0 6 –	Peterfburg.
		16	33	55	–	16	34	8	0 13 +	Pekin.
	9.	10	44	40	–	10	45	13	0 33 +	Greenw.
		12	46	30	–	12	46	27	0 3 –	Peterfburg.
	11.	7	15	30	–	7	15	36	0 6 +	Peterfb.
		12	59	19	–	13	0	1	0 42 +	Pekin.
	13.	7	28	57	–	7	29	12	0 15 +	Pekin.
	16.	12	50	34	–	12	51	2	0 28 +	Paris. Cl.
		13	12	37	–	13	12	19	0 18 –	Perinaldo.
	18.	7	19	48	–	7	20	5	0 17 +	Paris. Cl.
	*	7	41	33	–	7	41	22	0 11 –	Perinaldo.
		8	20	52	–	8	20	59	0 7 +	Tyrnav.
	20.	9	25	6	–	9	25	33	0 27 +	Pekin.
	25.	9	6	12	–	9	6	50	0 38 +	Greenw.
		9	15	35	–	9	16	8	0 33 +	Paris. Cl.
		9	15	49	–	9	16	6	0 17 +	Paris. O.
		9	37	1	–	9	37	31	0 30 +	Perinaldo.
		10	17	19	–	10	17	21	0 2 +	Upfala.
		10	18	43	–	10	19	1	0 18 +	Stockholm.
	27.	11	20	54	–	11	21	27	0 33 +	Pekin.
	29.	5	49	54	–	5	50	20	0 26 +	Pekin.
	Nov. 1.	11	2	10	–	11	2	25	0 15 +	Greenw.

Obfervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. M. D.	Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1773. Nov.	1.	11 11 49	Em.	11 11 43	0 6 —	Paris. Cl.
		11 33 24	—	11 33 0	0 24 —	Perinaldo.
	3.	6 23 58	—	6 23 58	0 0	Lund.
	26.	6 12 30	—	6 13 3	0 33 +	Perinaldo.
		6 35 2	—	6 35 13	0 11 +	Lund.
		6 52 57	—	6 52 59	0 2 +	Upſala.
		6 54 45	—	6 54 39	0 6 —	Stockh.
		7 44 4	—	7 43 42	0 22 —	Peterſburg. .
Dec.	3.	8 46 25	—	8 46 5	0 20 —	Upſala.
		8 47 58	—	8 47 45	0 13 —	Stockh.
	10.	9 27 59	—	9 28 15	0 16 +	Greenw.
	19.	6 59 15	—	6 59 11	0 4 —	Tyrnav.
1774. Jan.	2.	9 42 59	—	9 42 55	0 4 —	Paris. Cl.
	4.	5 12 7	—	5 12 4	0 3 —	Tyrnav.
	11.	6 4 24	—	6 4 10	0 14 —	Paris. Cl.
		7 7 2	—	7 7 3	0 1 +	Stockh.
	18.	7 48 22	—	7 48 20	0 2 —	Greenw.
Feb.	3.	6 15 33	—	6 15 8	0 25 —	Paris. Cl.
		7 16 17	—	7 16 21	0 4 +	Upſala.
		7 18 10	—	7 18 1	0 9 —	Stockh.
	19.	5 38 58	—	5 38 41	0 17 —	Stockh.
	26.	6 23 29	—	6 23 14	0 15 —	Greenw.
Aug.	4.	12 32 38	Im.	12 32 16	0 22 —	Tyrnav.
		12 32 51	—	12 32 35	0 16 —	Upſala.
		12 34 42	—	12 34 15	0 27 —	Stockh.
		13 23 17	—	13 23 18	0 1 +	Peterſburg. .
	18.	15 21 15	—	15 21 3	0 12 —	Paris. Cl.
	20.	10 51 10	—	10 51 6	0 4 —	Upſala.
		10 53 9	—	10 52 46	0 23 —	Stockh.
	27.	12 46 32	—	12 46 44	0 12 +	Upſala.
Sept.	3.	14 25 0	—	14 24 48	0 12 —	Lund.

Obſervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp.	Obfervationis			Calculus.			Diff. Calc.		Obfervatorium.
	M. D.	h	′	″	h	′	″	′	″	
1774.	Sept. 3.	14	42	21	Im. 14	42	15	0	6 —	Tyrnav.
		15	33	29	— 15	33	17	0	12 —	Peterſburg.
	10.	15	28	31	— 15	28	7	0	24 —	Greenw.
		16	38	28	— 16	38	19	0	9 —	Tyrnav.
	12.	9	57	14	— 9	57	13	0	1 —	Greenw.
		11	9	46	— 11	9	24	0	22 —	Stockh.
		11	58	36	— 11	58	27	0	9 —	Peterſb.
	21.	8	23	43	— 8	23	43	0	0	Peterſb.
	26.	13	49	53	— 13	49	40	0	13 —	Greenw.
		13	59	42	— 13	58	58	0	44 —	Paris. Cl.
Oct.	3.	15	46	19	— 15	45	47	0	32 —	Greenw.
		15	55	45	— 15	55	5	0	40 —	Paris. Cl.
		16	58	31	— 16	57	58	0	33 —	Stockh.
	5.	10	24	45	— 10	24	8	0	37 —	Paris. Cl.
		11	25	9	— 11	25	2	0	7 —	Tyrnav.
		11	26	59	— 11	27	1	0	2 +	Stockh. *d.*
		12	16	5	— 12	16	4	0	1 —	Peterſb.
	10.	17	41	57	— 17	41	43	0	14 —	Greenw.
	12.	12	20	25	— 12	20	0	0	25 —	Paris. Cl.
	14.	7	50	7	— 7	50	6	0	1 —	Upſala.
		7	52	0	— 7	51	46	0	14 —	Stockh.
	19.	14	16	20	— 14	15	32	0	48 —	Paris. Cl.
	21.	8	35	0	— 8	35	3	0	3 +	Greenw.
		8	44	47	— 8	44	21	0	26 —	Paris. Cl.
	26.	16	1	23	— 16	1	26	0	3 +	Greenw.
	30.	6	10	47	— 6	10	59	0	12 +	Stockh.
Nov.	11.	17	19	3	Em. 17	18	53	0	10 —	Lund.
	13.	12	5	2	— 12	4	39	0	23 —	Tyrnav.
	15.	6	15	39	— 6	15	36	0	3 —	Lund.
		6	34	34	— 6	35	2	0	28 +	Stockh.
	20.	12	56	42	— 12	57	3	0	21 +	Paris. Cl.

Obſervationes

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp. Obfervationis.					Calculus.			Diff. Calc. Obfervationis		
M. D.		h	′	″		h	′	″	′	″	
1774. Nov. 22.		8	27	22	Em.	8	28	10	0	48 +	Stockh.
Dec. 6.		11	9	25	—	11	10	7	0	42 +	Paris. C.
		12	11	9	—	12	11	1	0	8 —	Tyrnav.
24.		4	52	25	—	4	52	47	0	22 +	Stockh.
29.		11	3	48	—	11	4	32	0	44 +	Greenw.
		12	14	15	—	12	15	3	0	48 +	Upfala.
		12	16	11	—	12	16	43	0	32 +	Stockh
31.		6	25	5	—	6	25	17	0	12 +	Lund.
1775. Jan. 23.		6	48	55	—	6	49	43	0	48 +	Tyrnav.
Feb. 15.		5	53	2	—	5	53	32	0	30 +	Greenw.
22.		7	49	37	—	7	49	59	0	22 +	Greenw.
		8	42	39	—	8	42	44	0	5 +	Lund.
		9	0	6	—	9	0	11	0	5 +	Tyrnav.
		9	1	55	—	9	2	10	0	15 +	Stockh.
Mart. 10.		7	23	51	—	7	23	53	0	2 +	Tyrnav.
17.		8	11	7	—	8	11	21	0	14 +	Greenw.
Jul. 15.		14	42	36	Im.	14	42	14	0	22 —	Greenw.
24.		13	5	40	—	13	5	46	0	6 +	Petersb.
Aug. 7.		14	53	55	—	14	53	26.	0	29 —	Greenw.
16.		13	18	25	—	13	18	29	0	4 +	Petersb.
Sept. 1.		11	38	30	—	11	38	45	0	15 +	Petersb.
15.		15	31	7	—	15	30	54	0	13 —	Petersb.
24.		11	6	50	—	11	6	56	0	6 +	Stockh.
Oct. 1.		11	50	3	—	11	50	36	0	33 +	Greenw.
		13	3	2	—	13	2	47	0	15 —	Stockh.
10.		9	27	23	—	9	27	20	0	3 —	Stockh.
22.		17	37	1	—	17	36	25	0	36 —	Greenw.
Nov. 2.		8	27	48	—	8	27	35	0	13 —	Greenw.
18.		7	54	22	—	7	54	5	0	17 —	Stockh.
Dec. 11.		10	6	5	Em.	10	6	34	0	29 +	Stockh.
20.		6	23	33	—	6	23	37	0	4 +	Upfala.

Obfervationes comparatæ primi fatellitis Jovis.

Ann.	Temp.	Obfervationis				Calcu'us.			Diff. Calc.		Obfervatorium.
	M. D.	h	′	″		h	′	″	′	″	
1775.	Dec. 20.	6	24	55	Em.	6	25	17	0	22 +	Stockh.
	27.	7	3	6	—	7	4	9	1	3 +	Greenw.
		8	15	17	—	8	16	20	1	3 +	Stockh.
1776.	Jan. 12.	6	26	48	—	6	27	22	0	34 +	Stockh.
	19.	8	19	7	—	8	19	51	0	44 +	Stockh.
	26.	10	12	15	—	10	13	7	0	52 +	Stockh.
	28.	4	40	51	—	4	41	32	0	41 +	Stockh.
	Febr. 2.	12	6	20	—	12	7	7	0	47 +	Stockh.
	27.	6	51	5	—	6	51	33	0	28 +	Stockh.

XIV. *A Method of finding the Value of an infinite Series of decreasing Quantities of a certain Form, when it converges too slowly to be summed in the common Way by the mere Computation and Addition or Subtraction of some of its initial Terms.* By Francis Maseres, *Esquire, F. R. S. Cursitor Baron of the Exchequer.*

Read Feb. 13, 1777. **A**RTICLE Ift. Let *a, b, c, d, e, f, g, h,* &c. *ad infinitum,* reprefent a decreafing progreffion of numbers, fo that *b* fhall be lefs than *a,* and *c* than *b,* and *d* than *c,* and fo on of the following numbers, *ad infinitum.*

And 2dly, let thefe numbers be fo related to each other, that they not only fhall form a decreafing progreffion themfelves, but that their differences, *a−b, b−c, c−d, d−e, e−f, f−g, g−h,* &c. fhall alfo form a decreafing progreffion, fo that *b−c* fhall be lefs than *a−b,* and *c−d* than *b−c,* and *d−e* than *c−d,* and fo on of the following differences; and likewife, that the differences of thefe differences (which may be called *the fecond differences* of the original numbers *a, b, c, d, e, f, g, h,* &c. fhall form a decreafing progreffion; and that the differences of thofe fecond differences, or *the third differences* of the original

B b 2   numbers

numbers a, b, c, d, e, f, g, h, &c. ſhall alſo form a de-creaſing progreſſion; and in like manner, that the dif-ferences of the ſaid third differences, or *the fourth dif-ferences*, of the original numbers a, b, c, d, e, f, g, h, &c. and the fifth and ſixth differences, and all higher differ-ences, of the ſame numbers, ſhall alſo form decreaſing progreſſions.

And 3dly, let x be a quantity of any magnitude not greater than unity.

Upon theſe ſuppoſitions the value of the infinite ſeries $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 +$ &c. (in which the ſecond, fourth, ſixth, and eighth, and every following even term, is marked with the ſign $-$, or is to be ſub-tracted from that which immediately precedes it) may be determined in the following manner.

Art. 2. Compute the firſt, ſecond, third, fourth, and other ſubſequent differences of the co-efficients of the powers of x in this ſeries, that is, of the numbers b, c, d, e, f, g, h, &c. as far as ſhall be convenient. Theſe differences will be as follows.

First differences, $b-c$, $c-d$, $d-e$, $e-f$, $f-g$, $g-h$, &c.

Second differences,

$b-c-\overline{c-d}$, $c-d-\overline{d-e}$, $d-e-\overline{e-f}$, $e-f-\overline{f-g}$, $f-g-\overline{g-h}$, &c.

or, $b-2c+d$, $c-2d+e$, $d-2e+f$, $e-2f+g$, $f-2g+h$, &c.

Third differences,

$b-2c+d-\overline{c-2d+e}$, $c-2d+e-\overline{d-2e+f}$, $d-2e+f-\overline{e-2f+g}$,

$-2f+g-\overline{f-2g+h}$, &c.

or,

er, $b-3c+3d-e$, $c-3d+3e-f$, $d-3e+3f-g$, $e-3f+3g-h$, &c.

Fourth differences, $b-3c+3d-e-\overline{c-3d+3e-f}$,

$$c-3d+3e-f-\overline{d-3e+3f-g},$$

$$d-3e+3f-g-\overline{e-3f+3g-h}, \&c.$$

or, $b-4c+6d-4e+f$, $c-4d+6e-4f+g$, $d-4e+6f-4g+h$, &c.

Fifth differences, $b-4c+6d-4e+f-\overline{c-4d+6e-4f+g}$,

$$c-4d+6e-4f+g-\overline{d-4e+6f-4g+h}, \&c.$$

or, $b-5c+10d-10e+5f-g$, $c-5d+10e-10f+5g-h$, &c.

Sixth differences,

$$b-5c+10d-10e+5f-g-\overline{c-5d+10e-10f+5g-h}, \&c.$$

or, $b-6c+15d-20e+15f-6g+h$, &c.

Let the firſt difference of the firſt order, to wit, $b-c$, be called D^{I};

and the firſt difference of the ſecond order, to wit, $b-2c+d$, be called D^{II};

and the firſt difference of the third order, to wit, $b-3c+3d-e$, be called D^{III};

and the firſt difference of the fourth order, to wit, $b-4c+6d-4e+f$, be called D^{IV};

and the firſt difference of the fifth order, to wit, $b-5c+10d-10e+5f-g$, be called D^{V};

and the firſt difference of the ſixth order, to wit, $b-6c+15d-20e+15f-6g+h$, be called D^{VI};

and in like manner let the firſt differences of the ſeventh, eighth, ninth, and tenth, and every following order of differences be denoted by D^{VII}, D^{VIII}, D^{IX}, D^{X}, &c. that is, by the capital letter D, with a Roman numeral figure annexed.

annexed to it, expreffing the order of differences to which it belongs.

Thefe things being fuppofed, the aforefaid infinite feries $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^e +$ &c. will be equal to the following differential feries, to wit,

$$a - \frac{bx}{1+x} - \frac{D^I xx}{\overline{1+x}|^2} - \frac{D^{II} x^3}{\overline{1+x}|^3} - \frac{D^{III} x^4}{\overline{1+x}|^4} - \frac{D^{IV} x^5}{\overline{1+x}|^5} - \frac{D^V x^6}{\overline{1+x}|^5} - \frac{D^{VI} x^7}{\overline{1+x}|^7} - \&c.;$$

in which feries all the terms after the firft term a are marked with the fign $-$, or are to be fubtracted from that term.

Art. 3. If we infert the differences themfelves inftead of D^I, D^{II}, D^{III}, D^{IV}, D^V, &c. in the foregoing differential feries (which it may perhaps fometimes be convenient to do) that feries will be as follows: $a - \frac{bx}{1+x} - \overline{b-c} \times \frac{xx}{\overline{1+x}|^2}$

$$- \overline{b - 2c + d} \times \frac{x^3}{\overline{1+x}|^3} - \overline{b - 3c + 3d - e} \times \frac{x^4}{\overline{1+x}|^4}$$

$$- \overline{b - 4c + 6d - 4e + f} \times \frac{x^5}{\overline{1+x}|^5}$$

$$- \overline{b - 5c + 10d - 10e + 5f - g} \times \frac{x^6}{\overline{1+x}|^6}$$

$$- \overline{b - 6c + 15d - 20e + 15f - 6g + h} \times \frac{x^7}{\overline{1+x}|^7} - \&c.\ ad\ infini-$$

tum.

Of the convergency of the foregoing differential feries.

Art. 4. The foregoing differential feries will always converge with a confiderable degree of fwiftnefs, fo that

that fix or eight of its terms will give the value of the whole (and confequently that of the original feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. to which it is equal) exact to feveral places of figures, even in the moft difficult cafes: for if x is $= 1$ (which is its greateft poffible magnitude) $1+x$ will be $= 1+1$ or 2, and confequently $\overline{1+x}|^2$, $\overline{1+x}|^3$, $\overline{1+x}|^4$, $\overline{1+x}|^5$, and the following powers of $1+x$, will be equal to 4, 8, 16, 32, and the following powers of 2; and the powers of the fraction $\frac{x}{1+x}$ will be equal to the powers of $\frac{1}{2}$. Therefore the feries

$$a-\frac{bx}{1+x}-\frac{\mathrm{D^I}xx}{1+x|^2}-\frac{\mathrm{D^{II}}x^3}{1+x|^3}-\frac{\mathrm{D^{III}}x^4}{1+x|^4}-\frac{\mathrm{D^{IV}}x^5}{1+x|^5}-\frac{\mathrm{D^V}x^6}{1+x|^6}-\frac{\mathrm{D^{VI}}x^7}{1+x|^7}-\&c. \text{ will}$$

in this cafe be $=$ to $a-\frac{b}{2}-\frac{\mathrm{D^I}}{4}-\frac{\mathrm{D^{II}}}{8}-\frac{\mathrm{D^{III}}}{16}-\frac{\mathrm{D^{IV}}}{32}-\frac{\mathrm{D^V}}{64}-\frac{\mathrm{D^{VI}}}{128}-\&c.$

the terms of which decreafe in a greater proportion than that of 1 to 2, becaufe the numerators a, b, $\mathrm{D^I}$, $\mathrm{D^{II}}$, $\mathrm{D^{III}}$, $\mathrm{D^{IV}}$, $\mathrm{D^V}$, $\mathrm{D^{VI}}$, &c. form a decreafing progreffion, and the denominators increafe in the proportion of 2 to 1.

Of the inveftigation of the foregoing differential feries.

Art. 5. The foregoing differential feries was inveftigated by firft, fuppofing the original feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. to be equal to another feries whofe terms fhould involve the fame powers of x as the former, but in which every power of x fhould be multiplied

multiplied into the fame power of the fraction $\frac{1}{1+x}$, in order to accelerate their convergency, and then inquiring what would be the co-efficients of the terms of fuch a feries, if fuch a feries is poffible, and what would be the figns to be prefixed to them, or in what manner they would be connected with the firft term, whether by addition or fubtraction. In order to this inquiry, I denoted the unknown co-efficients of the affumed feries by the capital letters P, Q, R, S, T, V, &c. and wrote down the terms of it near each other, without prefixing to them either of the figns $+$ and $-$, but feparated them from each other only by a comma; fo that the fundamental equation, from which I derived the differential feries above-mentioned, was as follows: $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 +$ &c. is $= $ P, $\frac{Q x}{1+x}$, $\frac{R x x}{\overline{1+x}|^2}$, $\frac{S x^3}{\overline{1+x}|^3}$, $\frac{T x^4}{\overline{1+x}|^4}$, $\frac{V x^5}{\overline{1+x}|^5}$, &c. By neceffary deductions from this equation it appeared that P would be equal to a; and that all the following terms, of the affumed feries, to wit, $\frac{Q x}{1+x}$, $\frac{R x x}{\overline{1+x}|^2}$, $\frac{S x^3}{\overline{1+x}|^3}$, $\frac{T x^4}{\overline{1+x}|^4}$, $\frac{V x^5}{\overline{1+x}|^5}$, &c. muft be fubtracted from the firft term P, or a; and that Q would be equal to $b - c$, or D^I; and $R = b - c - \overline{c - d}$, or $b - 2c + d$, or D^{II}; and $S = b - 2c + d - \overline{c - 2d + e}$, or $b - 3c + 3d - e$, or D^{III}; and

$T =$

$\tau = b - 4c + 6d - 4e + f$, or D^{IV}; and $v = b - 5c + 10d - 10e$ $+ 5f - g$, or D^{V}; and fo on of the following co-efficients, to wit, that every new co-efficient of the affumed feries is equal to the firft difference of the next order of the differences derived from the original co-efficients b, c, d, e, f, g, h, &c. And from hence I concluded that the feries $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 +$ &c. was equal to the feries,

$$a - \frac{bx}{1+x} - \frac{D^I xx}{\overline{1+x}|^2} - \frac{D^{II} x^3}{\overline{1+x}|^3} - \frac{D^{III} x^4}{\overline{1+x}|^4} - \frac{D^{IV} x^5}{\overline{1+x}|^5} - \frac{D^V x^6}{\overline{1+x}|^6} - \frac{D^{VI} x^7}{\overline{1+x}|^7} - \&c.$$

Art. 6. The thought of fuppofing the original feries $a - bx + cxx - dx^3 + ex^4 - fx^5 +$ &c. to be equal to the feries P, $\frac{Qx}{1+x}$, $\frac{Rxx}{\overline{1+x}|^2}$, $\frac{Sx^3}{\overline{1+x}|^3}$, $\frac{Tx^4}{\overline{1+x}|^4}$, $\frac{Vx^5}{\overline{1+x}|^5}$, &c. containing the powers of x multiplied into the fame powers of the fraction $\frac{1}{1+x}$ in order to accelerate their convergency, occurred to me in confequence of reading the late Mr. THOMAS SIMPSON's Mathematical Differtations, p. 62, 63. concerning the fummation of feriefes, in which he makes a fuppofition of a fimilar kind. Yet there feems to be a confiderable difference between his propofition and that which is the fubject of thefe pages; for he feems to fuppofe his quantities p, q, r, s, t, &c. (which anfwer to a, b, c, d, e, &c. in the notation made ufe of in the above feriefes) to form an increafing progreffion of terms, and accordingly fubtracts p from q, and q from r,

and r from s, and s from t, and fo on; and he feems alfo to fuppofe the differences $q-p$, $r-q$, $s-r$, $t-s$, &c. to form an increafing progreffion, and every fubfequent order of differences to form likewife an increafing progreffion, and accordingly fubtracts $q-p$ from $r-q$, and $r-q$ from $s-r$, and $s-r$ from $t-s$, and fo on; whereas in the foregoing feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7$ + &c. the numbers a, b, c, d, e, f, g, h, &c. are fuppofed to form a decreafing progreffion of terms, as they are moft commonly found to do in the feriefes that occur in the folution of mathematical or philofophical problems.

Examples of the ufefulnefs of the foregoing differential feries in finding the values of infinite feriefes whofe terms decreafe very flowly.

Computations of the lengths of circular arcs by means of infinite feriefes derived from their tangents.

Art. 7. It is well known, that if r be put for the radius of a circle, and t for the tangent of any arch in it that is not greater than $45°$, the magnitude of the arch whofe tangent is t will be expreffed by the infinite feries

$$t-\frac{t^3}{3rr}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-\frac{t^{11}}{11r^{10}}+\frac{t^{13}}{13r^{12}}-\frac{t^{15}}{15r^{14}}+ \&c.$$ This feries converges with great fwiftnefs when the tangent is

much

much lefs than the radius; but when the tangent is nearly equal to the radius, it converges exceeding flowly; and when it is quite equal to the radius, or the arch is equal to 45°, the decreafe of the terms is fo flow as to make the computation of it in the common way, by computing the value of its initial terms, abfolutely impracticable. For Sir ISAAC NEWTON has obferved concerning this feries in that extreme cafe (which then becomes equal to $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} +$ &c.) and another feries that is almoft as flow as this, that to exhibit its value exact to twenty decimal places of figures, there would be occafion for no lefs than five thoufand millions of its terms, to compute which would take up above a thoufand years. See Sir ISAAC NEWTON's fecond letter to Mr. OLDENBURGH, dated October 24, 1676, in the *Commercium Epiftolicum*, p. 159. In thefe cafes therefore it will be convenient to make ufe of fome artifice to difcover the value of the feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c.; and we fhall find the application of the differential feries abovementioned to be a very proper artifice for this purpofe.

Art. 8. In order to make this application, we muft confider the feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. as being the product of the multiplication of t into the

feries

feries $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + $ &c. and muſt fubſtitute x inſtead of $\frac{tt}{rr}$ in the terms of this laſt feries, by which means it will be converted into the feries $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + $ &c. This feries is of the fame form with the original feries above-mentioned, $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 + $ &c. the numeral co-efficients $1, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}, \frac{1}{11}, \frac{1}{13}, \frac{1}{15}$, &c. of the powers of x in the former feries anſwering to the literal or general co-efficients a, b, c, d, e, f, g, h, &c. of the fame powers in the latter feries. And theſe numeral co-efficients evidently form a decreaſing progreſſion, as the co-efficients a, b, c, d, e, f, g, h, &c. are ſuppoſed to do; and we ſhall find, upon examination, that the differences of theſe numeral co-efficients, of the feveral fucceſſive orders, alſo conſtitute decreaſing progreſſions, as the feveral fucceſſive orders of differences of the co-efficients a, b, c, d, e, f, g, h, &c. are ſuppoſed to do. Confequently the feries $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + $ &c. will be equal to the differential feries

$$a - \frac{bx}{1+x} - \frac{D^I xx}{\overline{1+x}|^2} - \frac{D^{II}x^3}{\overline{1+x}|^3} - \frac{D^{III}x^4}{\overline{1+x}|^4} - \frac{D^{IV}x^5}{\overline{1+x}|^5} - \frac{D_V x^6}{\overline{1+x}|^6} - \frac{D^{VI}x^7}{\overline{1+x}|^7} - $$ &c. if

we ſuppoſe the letters a, b, c, d, e, f, g, h, &c. to be equal to the numbers $1, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}, \frac{1}{11}, \frac{1}{13}, \frac{1}{15}$, &c. and $D^I, D^{II}, D^{III}, D^{IV}, D^V, D^{VI}$, &c. to be the firſt differences of

the

the feveral orders of differences of thofe numbers, be-
ginning from the fecond term $\frac{1}{3}$.   Now the values of
thefe numbers, 1, $\frac{1}{3}$, $\frac{1}{5}$, $\frac{1}{7}$, $\frac{1}{9}$, $\frac{1}{11}$, $\frac{1}{13}$, $\frac{1}{15}$, &c. and of their
differences of the feveral fucceffive orders, beginning
from the fecond term $\frac{1}{3}$, will, when exprefſed in decimal.
fractions, be as follows:

$$1 \text{ is} = 1.000,000,000,000;$$
$$\tfrac{1}{3} = .333,333,333,333;$$
$$\tfrac{1}{5} = .200,000,000,000;$$
$$\tfrac{1}{7} = .142,857,142,857;$$
$$\tfrac{1}{9} = .111,111,111,111;$$
$$\tfrac{1}{11} = .090,909,090,909;$$
$$\tfrac{1}{13} = .076,923,076,923;$$
$$\tfrac{1}{15} = .066,666,666,666.$$

The differences of thefe numbers, beginning from the
fecond term, .333,333,333,33, are as follows:

First differences.	Second differences.
.133,333,333,333;	.076,190,476,190;
.057,142,857,143;	.025,396,825,397;
.031,746,031,746;	.011,544,011,544;
.020,202,020,202;	.006,216,006,216;
.013,986,013,986;	.003,729,603,729;
.010,256,410,257;	&c.
&c.	

Third

Third differences.

.050,793,650,793;
.013,852,813,853;
.005,328,005,328;
.002,486,402,487;
&c.

Fourth differences,

.036,940,836,940;
.008,524,808,525;
.002,841,602,841;
&c.

Fifth differences.

.028,416,028,415;
.005,683,205,684;
&c.

Sixth differences.

.022,732,822,731;
&c.

Therefore D^I is $= .133,333,333,333;$

$\qquad\qquad D^{II} \quad = .076,190,476,190;$

$\qquad\qquad D^{III} \quad = .050,793,650,793;$

$\qquad\qquad D^{IV} \quad = .036,940,836,940;$

$\qquad\qquad D^{V} \quad = .028,416,028,415;$

$\qquad\qquad D^{VI} \quad = .022,732,822,731.$

Therefore the feries $1 - \dfrac{x}{3} + \dfrac{xx}{5} - \dfrac{x^3}{7} + \dfrac{x^4}{9} - \dfrac{x^5}{11} + \dfrac{x^6}{13} - \dfrac{x^7}{15} + \&c.$ is equal to the feries

$$1 - .333,333,333,333, \times \frac{x}{1+x}$$

$$- .133,333,333,333, \times \frac{xx}{\overline{1+x}|^2}$$

$$- .076,190,476,190, \times \frac{x^3}{\overline{1+x}|^3}$$

$$- .050,793,650,793, \times \frac{x^4}{\overline{1+x}|^4}$$

$$- .036,$$

$$- .036,940,836,940, \times \frac{x^5}{\overline{1+x}|^5}$$

$$- .028,416,028,415, \times \frac{x^6}{\overline{1+x}|^6}$$

$$- .022,732,822,731, \times \frac{x^7}{\overline{1+x}|^7}$$

− &c.; and confequently the product of this latter feries into the tangent t will be equal to the product of the former feries $1-\frac{x}{3}+\frac{xx}{5}-\frac{x^3}{7}+\frac{x^4}{9}-\frac{x^5}{11}+\frac{x^6}{13}-\frac{x^7}{15}+$ &c. into the fame quantity, that is, to the product of the feries $1-\frac{tt}{3rr}+\frac{t^4}{5r^4}-\frac{t^6}{7r^6}+\frac{t^8}{9r^8}-\frac{t^{10}}{11r^{10}}+\frac{t^{12}}{13r^{12}}-\frac{t^{14}}{15r^{14}}+$ &c. into the tangent t, or to the original feries

$$t-\frac{t^3}{3rr}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-\frac{t^{11}}{11r^{10}}+\frac{t^{13}}{13r^{12}}-\frac{t^{15}}{15r^{14}}+ \text{ &c.}$$

which ex-preffes the magnitude of the arch of which t is the tangent.

Computation of an arch of 30 degrees.

Art. 9. Now let t be the tangent of 30°, which is $= r \times \frac{1}{\sqrt{3}}$. Then will tt be $= \frac{rr}{3}$; and $\frac{tt}{rr}$, or x, $= \frac{rr}{3rr}$, or $\frac{1}{3}$. Therefore $1+x$ will be $= 1+\frac{1}{3}=\frac{3}{3}+\frac{1}{3}=\frac{4}{3}$, and $\frac{x}{1+x}$ will be $= \frac{\frac{1}{3}}{\frac{4}{3}}=\frac{1}{3}\times\frac{3}{4}=\frac{1}{4}$. Therefore $\frac{xx}{\overline{1+x}|^2}$ will be $= \frac{1}{16}$, and $\frac{x^3}{\overline{1+x}|^3}$ $= \frac{1}{64}$, and $\frac{x^4}{\overline{1+x}|^4}=\frac{1}{256}$, and $x^5 = \frac{1}{1024}$, and $x^6 = \frac{1}{4096}$, and $x^7 = \frac{1}{16384}$. Confequently the differential feries will in this

cafe be equal to

$$1 - .333,333,333,333, \times \frac{1}{4}$$

$$- .133,333,333,333, \times \frac{1}{16}$$

$$- .076,190,476,190, \times \frac{1}{64}$$

$$- .050,793,650,793, \times \frac{1}{256}$$

$$- .036,940,836,940, \times \frac{1}{1024}$$

$$- .028,416,028,415, \times \frac{1}{4096}$$

$$- .022,732,822,731, \times \frac{1}{16384}$$

$$- \&c. = 1 - .083,333,333,333,$$

$$- .008,333,333,333,$$

$$- .001,190,476,190,$$

$$- .000,198,412,698,$$

$$- .000,036,075,036,$$

$$- .000,006,937,506,$$

$$- .000,001,387,501,$$

$$- \&c.$$

$$= 1 - .093,099,955,597, = 0.906,900,044,403.$$

Therefore the feries $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + \&c.$ or

$1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + \&c.$ is in this cafe

$= 0.906,900,044,403$, or (neglecting the latter figures after the fixth place of figures, becaufe we are fure they

are

are not exact) 0.906,900. Therefore the product of the

series $1 - \dfrac{tt}{3rr} + \dfrac{t^4}{5r^4} - \dfrac{t^6}{7r^6} + \dfrac{t^8}{9r^8} - \dfrac{t^{10}}{11r^{10}} + \dfrac{t^{12}}{13r^{12}} - \dfrac{t^{14}}{15r^{14}} +$ &c. into the

tangent t is equal to $0.906,900, \times t = 0.906,900 \times r \times \dfrac{1}{\sqrt{3}}$

$= 0.906,900, \times r \times \dfrac{1}{1.732,050,8} = 0.906,900 \times r$

$\times .577,350,2 = 0.523,598,8 \times r$; that is, the series

$t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^5} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}} + \dfrac{t^{13}}{13r^{12}} - \dfrac{t^{15}}{15r^{14}} +$ &c. (which ex-

presses the magnitude of the arch of which t is the tan-

gent) is in this case $= 0.523,598,8 \times r$, or an arch of $30^{\cdot}$

is equal to $0.523,598,8 \times r$.

Art. 10. This value of an arch of $30°$ is exact in the

six first places of figures, and errs only an unit in the se-

venth figure, which should be a 7 instead of an 8, the

more exact value of that arch being $0.523,598,775,598$,

&c.   And thus by the help of only eight terms of the

differential series

$a - \dfrac{bx}{1+x} - \dfrac{\mathrm{D^I}xx}{1+x|^2} - \dfrac{\mathrm{D^{II}}x^3}{1+x|^3} - \dfrac{\mathrm{D^{III}}x^4}{1+x|^4} - \dfrac{\mathrm{D^{IV}}x^5}{1+x|^5} - \dfrac{\mathrm{D^V}x^6}{1+x|^6} - \dfrac{\mathrm{D^{VI}}x^7}{1+x|^7} -$ &c.   we

have obtained the value of the series

$t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}} + \dfrac{t^{13}}{13r^{12}} - \dfrac{t^{15}}{15r^{14}} +$ &c. in the case of

an arch of 30 degrees, exact to six places of figures.

This degree of exactness is the same with that which we

should attain by computing twelve terms of the series

$t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}} + \dfrac{t^{13}}{13r^{12}} - \dfrac{t^{15}}{15r^{14}} +$ &c. itself, as will

appear from the following calculation.

Art. 11. The feries $t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}} + \dfrac{t^{13}}{13r^{12}}$

$- \dfrac{t^{15}}{15r^{14}} + \dfrac{t^{17}}{17r^{16}} - \dfrac{t^{19}}{19r^{18}} + \dfrac{t^{21}}{21r^{20}} - \dfrac{t^{23}}{23r^{22}} + $ &c. is $= t \times$ the feries

$1 - \dfrac{tt}{3rr} + \dfrac{t^4}{5r^4} - \dfrac{t^6}{7r^6} + \dfrac{t^8}{9r^5} - \dfrac{t^{10}}{11r^{10}} + \dfrac{t^{12}}{13r^{12}} - \dfrac{t^{14}}{15r^{14}} + \dfrac{t^{16}}{17r^{16}} - \dfrac{t^{18}}{19r^{18}} + \dfrac{t^{20}}{21r^{20}}$

$- \dfrac{t^{22}}{23r^{22}} + $ &c. =, in the cafe of an arch of 30°, to $r \times \dfrac{1}{\sqrt{3}}$ into

the feries $1 - \dfrac{1}{3\times3} + \dfrac{1}{5\times9} - \dfrac{1}{7\times27} + \dfrac{1}{9\times81} - \dfrac{1}{11\times243} + \dfrac{1}{13\times729}$

$- \dfrac{1}{15\times2187} + \dfrac{1}{17\times6561} - \dfrac{1}{19\times19683} + \dfrac{1}{21\times59049} - \dfrac{1}{23\times177,147} + $ &c.

$= r \times \dfrac{1}{\sqrt{3}} \times$ the feries $1.000,000,000,000, - \dfrac{.333,333,333,333}{3}$

$\qquad + \dfrac{.111,111,111,111,}{5} - \dfrac{.037,037,037,037,}{7}$

$\qquad + \dfrac{.012,345,679,012,}{9} - \dfrac{.004,115,226,337,}{11}$

$\qquad + \dfrac{.001,371,742,112,}{13} - \dfrac{.000,457,247,370,}{15}$

$\qquad + \dfrac{.000,152,415,790,}{17} - \dfrac{.000,050,805,263,}{19}$

$\qquad + \dfrac{.000,016,935,087,}{21} - \dfrac{.000,005,645,029,}{23}$

$\qquad + $ &c.

$= r \times \dfrac{1}{\sqrt{3}} \times$ the feries

$\quad 1.000,000,000,000, - .111,111,111,111,$

$\quad + .022,222,222,222, - .005,291,005,291,$

$\quad + .001,371,742,112, - .000,374,111,485,$

$\quad + .000,105,518,624, - .000,030,483,158,$

$\quad + .000,008,965,634, - .000,002,673,961,$

$\quad + .000,000,806,432, - .000,000,245,436,$

$\quad + $ &c.

I

$$= r \times \frac{1}{\sqrt{3}} \times \overline{1.023,709,255,024, - .116,809,620.\overline{442,}}$$
$$+ \&c.$$

$$= r \times \frac{1}{\sqrt{3}} \times .906,899,624,582,$$

$$= r \times \frac{1}{1\,732,050,8} \times .906,899,624,582,$$

$= r \times .577,350,2 \times .906,899,624,582, =$ (if we neglect the six latter figures of $906,899,624,582$, which we know to be not exact) $r \times .577,350,2 \times 906,899,$ $= r \times .523,598,319,029,8$; of which the first six figures .$523,598$, are exact.

Computation of an arch of 45 degrees.

Art. 12. Now let the tangent t be equal to the radius r, or the arch (whose magnitude is expressed by the series $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + $ &c.) be an arch of 45°. This series will, in this case, become equal to $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} + $ &c. of which the first eight terms will give the value of the whole exact to only one figure, as will appear by the following computation. These terms are equal to $r \times$ the eight terms $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \frac{1}{13} - \frac{1}{15}$, that is, to $r \times$ the eight terms

$$1.000,000,000,000, - .333,333,333,333,$$
$$+ .200,000,000,000, - .142,857,142,857,$$
$$+ .111,111,111,111, - .090,909,090,909,$$
$$+ .076,923,076,923, - .066,666,666,666,$$

$$= r \times \overline{1.388,034,187,034,} - .633,766,233,765,$$

$= r \times .754,267,943,269;$ which agrees with the value

of the whole feries $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} +$ &c. only in the

higheft figure 7, the more exact value of that feries

being .785,398,163,397, &c. But, if we compute eight

terms of the differential feries which is equal to the

feries $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} +$ &c., we fhall thereby obtain

its value exact to three places of figures; which is as

great a degree of exactnefs as would be attained by

computing about five hundred terms of the feries

$r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} +$ &c. itfelf. The computation

of the eight firft terms of the faid differential feries is as

follows.

Art. 13. Since t is in this cafe $= r$, tt will be $= rr$,

and confequently $\frac{tt}{rr}$, or x, will be $= 1$. Therefore xx, x^3,

x^4, x^5, and all the other powers of x, will in this cafe be

equal to 1, and $1 + x$ will be equal to $1 + 1$, or 2, and the

powers of $1 + x$ to the powers of 2. Therefore the frac-

tion $\frac{x}{1+x}$ and its powers will be equal in this cafe to the

fraction $\frac{1}{2}$ and its powers. Therefore the general differ-

ential feries in art. 8. to wit,

$$1 - .333,333,333,333, \times \frac{x}{1+x}$$

$$- .133,333,333,333, \times \frac{xx}{\overline{1+x}|^2}$$

$$- .076,190,476,190, \times \frac{x^3}{\overline{1+x}|^3}$$

$$- .050,793,650,793, \times \frac{x^4}{\overline{1+x}|^4}$$

$$- .036,940,836,940, \times \frac{x^5}{\overline{1+x}|^5}$$

$$- .028,416,028,415, \times \frac{x^6}{\overline{1+x}|^6}$$

$$- .022,732,822,731, \times \frac{x^7}{\overline{1+x}|^7}$$

&c. will become in this cafe equal to

$$1 - .333,333,333,333, \times \frac{1}{2}$$

$$- .133,333,333,333, \times \frac{1}{4}$$

$$- .076,190,476,190, \times \frac{1}{8}$$

$$- .050,793,650,793, \times \frac{1}{16}$$

$$- .036,940,836,940, \times \frac{1}{32}$$

$$- .028,416,028,415, \times \frac{1}{64}$$

$$- .022,732,822,731, \times \frac{1}{128}$$

$$- \&c. = 1 - .166,666,666,666,$$

$$- .033,333,333,333,$$

$$- .009,523,809,523,$$

$$- .003,174,603,174,$$

$$- .001,154,401,154,$$

$$- .000,444,000,443,$$

$$- .000,177,600,177,$$

$$- \&c.$$

$$= 1 - .214,474,414,470, = .785,525,585,530.$$

Therefore the series $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + \&c.$ or

$1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + \&c.$ is equal to

.785,525,585,530; and consequently the series

$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.$ is in this case

$= t \times .785,525,585,530, = r \times .785,525,585,530;$ that
is, the length of an arch of 45°, in a circle whose ra-
dius is r, is $= r \times .785,525,585,530;$ which number is
true to three places of figures, the more exact value of
that arch being $r \times .785,398,163,397,$ &c.

Art. 14. It has been asserted in art. 12. that in
order to obtain the value of the series

$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.$ exact to 3

places of decimal figures by the mere computation of its
terms, in the case of an arch of 45°, we must compute
at least 500 of its terms. This may be proved in the
following manner. The indexes of the powers of t in
that series are the odd numbers 1, 3, 5, 7, 9, 11, 13, 15,
&c.

&c. in their natural order; to which if we add an unit, the numbers thereby produced will be the even numbers 2, 4, 6, 8, 10, 12, 14, 16, &c. in their natural order, which are the doubles of the natural numbers, 1, 2, 3, 4, 5, 6, 7, &c. Therefore the number of terms of that feries from the beginning of it to any given term in it, including the faid term, is always half the number that is produced by adding an unit to the index of t in the faid term. Thus, if we take the term $\frac{t^{11}}{11 r^{10}}$, and add 1 to 11, which is the index of the power of t in it, the fum will be 12, the half of which is 6, which is the number of terms in the feries from the beginning of it to the term $\frac{t^{11}}{11 r^{10}}$, including the faid term, that term being the fixth term in the feries.   If therefore we take the term $\frac{t^{999}}{999 \, r^{998}}$, and are defirous of knowing its place in the feries, or the number of terms from the beginning of the feries to that term inclufively, we muft add 1 to the index of the power of t in its numerator, which will increafe it to 1000; and half this fum, to wit, 500, will be the number of terms from the beginning of the feries to the term $\frac{t^{999}}{999 \, r^{998}}$ inclufively; or, in other words, this term will be the 500th term of the feries. To arrive therefore at thofe terms of the feries in which the in-

dexes

dexes of the powers of t are greater than 999, or 1000, or in which the numeral co-efficients of the terms (which, by the law of this feries, are equal to 1 divided by thefe indexes) are lefs than $\frac{1}{999}$ or $\frac{1}{1000}$, it is neceffary to compute 500 of its terms. Now when t is $= r$, and confequently the literal parts of the terms of this feries do not converge at all, it is evidently neceffary to carry the computation as far as thofe terms in which the numeral co-efficients of the terms are lefs than $\frac{1}{999}$ or $\frac{1}{1000}$, in order to get the value of the feries exact to the $\frac{1}{999}$th or $\frac{1}{1000}$th part of the radius r, or to the place of thoufandths, or the third place of decimal figures. Therefore, when t is $= r$, or the arch is $= 45°$, it is neceffary to compute at leaft 500 terms of the feries

$$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.,$$

in order to obtain the value of it exact to three places of decimal figures, that is, to the fame degree of exactnefs to which we attained in art. 13. by computing only eight terms of the above-mentioned differential feries. *Q.E.D.*

Art. 15. But the beft way of applying the aforefaid differential feries to the inveftigation of the value of one of thefe very flow feriefes, is to compute a moderate number of the firft terms of the flow feries in the common way, and then apply the differential feries to the

computation of its remaining terms. The advantage of this method of proceeding will be manifeſt, if we apply it to the foregoing example of the ſeries

$$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^3} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.$$ in the caſe of an arch of 45°.

Compute therefore the firſt twelve terms of this ſeries in the common way. Theſe terms will be as follows:

$$t = r = r \times 1.000,000,000,000;$$

$$\frac{t^3}{3rr} = \frac{r^3}{3rr} = \frac{r}{3} = r \times .333,333,333,333;$$

$$\frac{t^5}{5r^4} = \frac{r^5}{5r^4} = \frac{r}{5} = r \times .200,000,000,000;$$

$$\frac{t^7}{7r^6} = \frac{r^7}{7r^6} = \frac{r}{7} = r \times .142,857,142,857;$$

$$\frac{t^9}{9r^3} = \frac{r^9}{9r^8} = \frac{r}{9} = r \times .111,111,111,111;$$

$$\frac{t^{11}}{11r^{10}} = \frac{r^{11}}{11r^{10}} = \frac{r}{11} = r \times .090,909,090,909;$$

$$\frac{t^{13}}{13r^{12}} = \frac{r^{13}}{13r^{12}} = \frac{r}{13} = r \times .076,923,076,923;$$

$$\frac{t^{15}}{15r^{14}} = \frac{r^{15}}{15r^{14}} = \frac{r}{15} = r \times .066,666,666,666;$$

$$\frac{t^{17}}{17r^{16}} = \frac{r^{17}}{17r^{16}} = \frac{r}{17} = r \times .058,823,529,411;$$

$$\frac{t^{19}}{19r^{18}} = \frac{r^{19}}{19r^{18}} = \frac{r}{19} = r \times .052,631,578,947;$$

$$\frac{t^{21}}{21r^{20}} = \frac{r^{21}}{21r^{20}} = \frac{r}{21} = r \times .047,619,047,619;$$

$$\frac{t^{23}}{23r^{22}} = \frac{r^{23}}{23r^{22}} = \frac{r}{23} = r \times .043,478,260,869.$$

Therefore the twelve terms $t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}}$

$+ \dfrac{t^{13}}{13r^{12}} - \dfrac{t^{15}}{15r^{14}} + \dfrac{t^{17}}{17r^{16}} - \dfrac{t^{19}}{19r^{18}} + \dfrac{t^{21}}{21r^{20}} - \dfrac{t^{23}}{23r^{22}}$ are

$$
= \left\{
\begin{aligned}
& r \times 1.000,000,000,000, - r \times .333,333,333,333, \\
& + r \times\ \ .200,000,000,000, - r \times .142,857,142,857, \\
& + r \times\ \ .111,111,111,111, - r \times .090,909,090,909, \\
& + r \times\ \ .076,923,076,923, - r \times .066,666,666,666, \\
& + r \times\ \ .058,823,529,411, - r \times .052,631,578,947, \\
& + r \times\ \ .047,619,047,619, - r \times .043,478,260,869,
\end{aligned}
\right\}
$$

$= r \times 1.494,476,765,064, - r \times .729,876,073,581,$

$= r \times\ \ .764,600,691,483.$

Having thus found the value of the firſt twelve terms

of the ſeries $t - \dfrac{t^3}{3rr} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} + \dfrac{t^9}{9r^8} - \dfrac{t^{11}}{11r^{10}} +$ &c. to be

$r \times .764,600,691,483$, we muſt apply the differential ſeries to the diſcovery of the value of the remaining part of this ſeries, which is the ſeries

$\dfrac{t^{25}}{25r^{24}} - \dfrac{t^{27}}{27r^{26}} + \dfrac{t^{29}}{29r^{28}} - \dfrac{t^{31}}{31r^{30}} + \dfrac{t^{33}}{33r^{32}} - \dfrac{t^{35}}{35r^{34}} + \dfrac{t^{37}}{37r^{36}} - \dfrac{t^{9}}{39r^{38}} +$ &c. *ad in-*

finitum. Now this ſeries is equal to the product of $\dfrac{t^{25}}{r^{24}}$ into

the ſeries $\dfrac{1}{25} - \dfrac{tt}{27rr} + \dfrac{t^4}{29r^4} - \dfrac{t^6}{31r^6} + \dfrac{t^8}{33r^8} - \dfrac{t^{10}}{35r^{10}} + \dfrac{t^{12}}{37r^{12}} - \dfrac{t^{14}}{39r^{14}} +$ &c. or

(putting x, as before, $= \dfrac{tt}{rr}$) to the product of $\dfrac{t^{25}}{r^{24}}$ into the

　　　　　　　　　　　　　　　　　　　　　. ſeries

feries $\frac{1}{25} - \frac{x}{27} + \frac{xx}{29} - \frac{x^3}{31} + \frac{x^4}{33} - \frac{x^5}{35} + \frac{x^6}{37} - \frac{x^7}{39} +$ &c. which is of the fame form with the feries $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 +$ &c. Therefore, if we put $a = \frac{1}{25}$, $b = \frac{1}{27}$, $c = \frac{1}{29}$, $d = \frac{1}{31}$, $e = \frac{1}{33}$, $f = \frac{1}{35}$, $g = \frac{1}{37}$, $h = \frac{1}{39}$, and fo on, and compute the differential feries

$$a - \frac{bx}{1+x} - \frac{\mathrm{D}^{\mathrm{I}}xx}{\overline{1+x}|^2} - \frac{\mathrm{D}^{\mathrm{II}}x^3}{\overline{1+x}|^3} - \frac{\mathrm{D}^{\mathrm{III}}x^4}{\overline{1+x}|^4} - \frac{\mathrm{D}^{\mathrm{IV}}x^5}{\overline{1+x}|^5} - \frac{\mathrm{D}^{\mathrm{V}}x^6}{\overline{1+x}|^6} - \frac{\mathrm{D}^{\mathrm{VI}}x^7}{\overline{1+x}|^7} - \text{ &c.}$$

thence refulting, the number thereby obtained will be the value of the feries $\frac{1}{25} - \frac{x}{27} + \frac{xx}{29} - \frac{x^3}{31} + \frac{x^4}{33} - \frac{x^5}{35} + \frac{x^6}{37} - \frac{x^7}{39} +$ &c. or $\frac{1}{25} - \frac{tt}{27rr} + \frac{t^4}{29r^4} - \frac{t^6}{31r^6} + \frac{t^8}{33r^8} - \frac{t^{10}}{35r^{10}} + \frac{t^{12}}{37r^{12}} - \frac{t^{14}}{39r^{14}} +$ &c. This computation is as follows:

Here a is $= \frac{1}{25} = .040,000,000,000$;

$b \quad = \frac{1}{27} = .037,037,037,037$;

$c \quad = \frac{1}{29} = .034,482,758,620$;

$d \quad = \frac{1}{31} = .032,258,064,516$;

$e \quad = \frac{1}{33} = .030,303,030,303$;

$f \quad = \frac{1}{35} = .028,571,428,571$;

$g \quad = \frac{1}{37} = .027,027,027,027$;

$h \quad = \frac{1}{39} = .025,641,025,641$.

The

The differences of these numbers (beginning from the second number $\frac{1}{27}$, or .037,037,037,037,) are as follows:

Firſt differences.

.002,554,278,416;
.002,224,694,104;
.001,955,034,213;
.001,731,601,731;
.001,544,401,544;
.001,386,001,386.

Second differences.

.000,329,584,311;
.000,269,659,891;
.000,223,432,481;
.000,187,200,187;
.000,158,400,158.

Third differences.

.000,059,924,420;
.000,046,227,409;
.000,036,232,294;
.000,028,800,028.

Fourth differences.

.000,013,697,010;
.000,009,995,115;
.000,007,432,265.

Fifth differences.

.000,003,701,894;
.000,002,562,850.

Sixth differences.

.000,001,139,044.

Therefore D^I is $= .002,554,278,416;$

$$D^{II} \quad = .000,329,584,311;$$
$$D^{III} \quad = .000,059,924,420;$$
$$D^{IV} \quad = .000,013,697,010;$$
$$D^{V} \quad = .000,003,701,894;$$
$$D^{VI} \quad = .000,001,139,044.$$

Conſequently the differential ſeries

$$a - \frac{bx}{1+x} - \frac{D^I xx}{\overline{1+x}|^2} - \frac{D^{II} x^3}{\overline{1+x}|^3} - \frac{D^{III} x^4}{\overline{1+x}|^4} - \frac{D^{IV} x^5}{\overline{1+x}|^5} - \frac{D^V x^6}{\overline{1+x}|^6} - \frac{D^{VI} x^7}{\overline{1+x}|^7} - \&c. \text{ is} = \text{to}$$

.040,

.040,000,000,000,

$- .037,037,037,037, \times \frac{x}{1+x}$

$- .002,554,278,416, \times \frac{xx}{\overline{1+x}|^2}$

$- .000,329,584,311, \times \frac{x^3}{\overline{1+x}|^3}$

$- .000,059,924,420, \times \frac{x^4}{\overline{1+x}|^4}$

$- .000,013,697,010, \times \frac{x^5}{\overline{1+x}|^5}$

$- .000,003,701,894, \times \frac{x^6}{\overline{1+x}|^6}$

$- .000,001,139,044, \times \frac{x^7}{\overline{1+x}|^7}$

$-$ &c.

But, fince t is in this cafe $= r; \frac{tt}{rr}$, or x, will be $=1$, and confequently $\frac{x}{1+x}$ will be $= \frac{1}{1+1}$ or $\frac{1}{2}$. Therefore the foregoing differential feries is in this cafe equal to

.040,000,000,000,

$- .037,037,037,037, \times \frac{1}{2}$

$- .002,554,278,416, \times \frac{1}{4}$

$- .000,329,584,311, \times \frac{1}{8}$

$- .000,059,924,420, \times \frac{1}{16}$

$- .000,013,697,010, \times \frac{1}{32}$

$- .000,$

$$- .000,003,701,894, \times \frac{1}{64}$$

$$- .000,001,139,044, \times \frac{1}{128}$$

$$- \&c. = .040,000,000,000, - .018,518,518,518,$$
$$- .000,638,569,604,$$
$$- .000,041,198,038,$$
$$- .000,003,745,276,$$
$$- .000,000,428,031,$$
$$- .000,000,057,842,$$
$$- .000,000,008,898,$$
$$- \&c.$$

$$= .040,000,000,000, - .019,202,526,207, - \&c.$$

$$= .020,797,473,793 - \&c.$$

Therefore the feries $\frac{1}{25} - \frac{x}{27} + \frac{xx}{29} - \frac{x^3}{31} + \frac{x^4}{33} - \frac{x^5}{35} + \frac{x^6}{37} - \frac{x^7}{39} + $ &c. or

$\frac{1}{25} - \frac{tt}{27rr} + \frac{t^4}{29r^4} - \frac{t^6}{31r^6} + \frac{t^8}{33r^8} - \frac{t^{10}}{35r^{10}} + \frac{t^{12}}{37r^{12}} - \frac{t^{14}}{39r^{14}} + $ &c. is in this

cafe $= .020,797,473,793$. Therefore the feries

$\frac{t^{25}}{25r^{24}} - \frac{t^{27}}{27r^{26}} + \frac{t^{29}}{29r^{28}} - \frac{t^{31}}{31r^{30}} + \frac{t^{33}}{33r^{32}} - \frac{t^{35}}{35r^{34}} + \frac{t^{37}}{37r^{36}} - \frac{t^{39}}{39r^{38}} + $ &c. is in

this cafe equal to $\frac{t^{25}}{r^{24}} \times .020,797,473,793$, that is, to

$\frac{r^{25}}{r^{24}} \times .020,797,473,793$, or $r \times .020,797,473,793$; that

is, the remainder of the infinite feries

$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + $ &c. after the firft twelve

terms, is $= r \times .020,797,473,793$. But we before found

7   thofe

thofe firſt twelve terms to be $= r \times .764{,}600{,}691{,}483$.
Therefore the whole feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} +$
&c. *ad infinitum* is in this cafe $= r \times .764{,}600{,}691{,}483$,

   $+ r \times .020{,}797{,}473{,}793, = r \times .785{,}398{,}165{,}276$,

which is true to eight places of figures, the more exact
value of that feries being $r \times .785{,}398{,}163{,}397$, &c.;
fo that the value here found for this feries, by the help
of only eight terms of the differential feries, differs from
its true value by lefs than an unit in the eighth place of
decimal figures, that is, by lefs than an hundred-mil-
lionth part of the radius r, which is a degree of exact-
nefs that could not have been attained by the mere com-
putation of the feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} +$ &c. it-
felf without computing fifty millions of its terms. There
cannot be a ſtronger inſtance of the utility of that dif-
ferential feries.

*Computation of the feries which expreſſes the time of the
defcent of a pendulum through the arch of a circle.*

Art. 16. As another example of the utility of the
foregoing differential feries in finding the value of a fe-
ries that converges very flowly, I will now apply it to
the feries which exprefſes the time of defcent of a
heavy body through a circular arch of 90°, which de-

                                        creafes

creafes almoft as flowly as the above-mentioned feries

$$r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} + \text{ \&c.,}$$

which expreffes the magnitude of a circular arch of 45° in a circle whofe radius is r.

Art. 17. If a heavy body, or a pendulum, be fuppofed to defcend by the mere force of gravity through any arch of a circle not exceeding the arch of a quadrant, or 90°; and the motion be fuppofed to begin from a ftate of reft, and to continue till the bob of the pendulum, or the heavy body, comes to the loweft point of the circle; and the radius of the circle be called r, the perpendicular height, or verfed fine, of the arch through which the defcent is made, be called v, and the right fine of the fame arch be called s; and π be put for the number 1.570,796,326,794, &c. which expreffes the femi-circumference of a circle whofe diameter is called 1; and the time of the fall of a heavy body through the verfed fine v, or the perpendicular altitude of the arch through which the pendulum defcends, be denoted by v; the time of the defcent of the pendulum through the faid circular arch, correfponding to the verfed fine or altitude v, to the loweft point of the circle, will be expreffed by the product of $\pi \times \dfrac{rv}{s}$ into the feries

$$1 - \frac{1.1}{2.2} \times \frac{vv}{ss} + \frac{1.1 \ 3.3}{2 \ 2.4.4} \times \frac{v^4}{s^4} - \frac{1.1.3.3.5.5}{2.2.4.4.6.6} \times \frac{v^6}{s^6} + \frac{1.1.3 \ 3.5.5.7.7}{2.2.4.4.6.6.8.8} \times \frac{v^8}{s^8}$$

$$-\frac{1.1.3.3.5.5.7.7.9.9}{2.2.4.4.6.6.8.8.10.10} \times \frac{v^{10}}{s^{10}} + \&c.$$ in which the law of the continuation of the terms is very manifeft, every new term being derived from the preceding term by multiplying it into the fraction $\frac{vv}{ss}$, and likewife into a numeral fraction, whofe denominator is the fquare of the index of the powers of v and s in the new term, and whofe numerator is the fquare of the odd number that is lefs than the faid index by an unit.

Art. 18. Let the numeral co-efficients of the terms of this feries be denoted by the capital letters of the alphabet, A, B, C, D, E, F, G, H, &c. in their natural order, fo that A fhall be equal to 1, and B fhall be $= \frac{1.1}{2.2}$, and $C = \frac{1.1.3.3}{2.2.4.4}$, and $D = \frac{1.1.3.3.5.5}{2.2.4.4.6.6}$, and fo on of the reft. And we fhall have

$$B = \frac{1.1}{2.2}A, \quad C = \frac{3.3}{4.4}B, \quad D = \frac{5.5}{6.6}C, \quad E = \frac{7.7}{8.8}D, \quad F = \frac{9.9}{10.10}E, \quad G = \frac{11.11}{12.12}F, \quad H = \frac{13.13}{14.14}G,$$

and fo on; and confequently the feries

$$1 - \frac{1.1}{2.2} \times \frac{vv}{ss} + \frac{1.1.3.3}{2.2.4.4} \times \frac{v^4}{s^4} - \frac{1.1.3.3.5.5}{2.2.4.4.6.6} \times \frac{v^6}{s^6} + \&c.$$

or $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}} + \frac{Gv^{12}}{s^{12}} - \frac{Hv^{14}}{s^{14}} + \&c.$ will be A −

$$\frac{1.1.Avv}{2.2.\ ss} + \frac{3.3.Bv^4}{4.4.\ s^4} - \frac{5.5.Cv^6}{6.6.\ s^6} + \frac{7.7\ Dv^8}{8.8.\ s^8} - \frac{9.9.\ Ev^{10}}{10.10.\ s^{10}} + \frac{11.11.Fv^{12}}{12.12.\ s^{12}} - \frac{13.13.Gv^{14}}{14.14.\ s^{14}}$$

+ &c. or $A - \frac{1Avv}{4ss} + \frac{9Bv^4}{16s^4} - \frac{25Cv^6}{36s^6} + \frac{49Dv^8}{64s^8} - \frac{81Ev^{10}}{100s^{10}} + \frac{121Fv^{12}}{144s^{12}} - \frac{169Gv^{14}}{196s^{14}}$

+ &c. or, if we convert the co-efficients of the terms into decimal fractions,

$$1 - .250,000,000,000,000, \times \frac{vv}{ss} + .140,625,000,000, \times \frac{v^4}{s^4}$$

$$-.097,656,250,000, \times \frac{v^6}{s^6} + .074,768,066,406, \times \frac{v^8}{s^8}$$

$$-.060,562,133,788, \times \frac{v^{10}}{s^{10}} + .050,889,015,196, \times \frac{v^{12}}{s^{12}}$$

$$-.043,878,793,714, \times \frac{v^{14}}{s^{14}} + \&c.$$ The co-efficients of
thefe terms decreafe fo flowly (efpecially after the firſt
twelve or fourteen terms) that, when the verfed fine v is
very nearly equal to the right fine s (as is the cafe when the
arch through which the heavy body defcends is nearly
equal to 90°, or the arch of a whole quadrant of a circle)
it would be neceffary to compute a vaſt number of the
terms of the feries in order to obtain its value exact to
feven or eight places of figures; and, when v is quite
equal to s (as is the cafe when the arch, through which
the defcent is made, is exactly equal to 90°) the compu-
tation of the value of the feries to that degree of exact-
nefs in that direct manner becomes wholly impractica-
ble.   But by the help of the differential feries above-
mentioned its value may be found, even in this cafe, to
that degree of exactnefs without much difficulty; more
efpecially if we compute the firſt twelve terms of the
feries in the common way, and then apply the differen-
tial feries to the inveſtigation of the remaining part of it
in the fame manner as in the laſt example. This we
fhall now proceed to do.

5   Art.

Art. 19. The co-efficients of the firſt twelve terms of

the feries $A - \dfrac{1 A vv}{4ss} + \dfrac{9 B v^4}{16 s^4} - \dfrac{25 C v^6}{36 s^6} + \dfrac{49 D v^8}{64 s^8} - \dfrac{81 E v^{10}}{100 s^{10}} + \dfrac{121 F v^{12}}{144 s^{12}}$

$- \dfrac{169 G v^{14}}{196 s^{14}} +$ &c. are as follows:

$$A = 1 = 1.000,000,000,000;$$

$$B = \frac{1}{4} = .250,000,000,000;$$

$$C = \frac{9 B}{16} = .140,625,000,000;$$

$$D = \frac{25 C}{36} = .097,656,250,000;$$

$$E = \frac{49 D}{64} = .074,768,066,406;$$

$$F = \frac{81 E}{100} = .060,562,133,788;$$

$$G = \frac{121 F}{144} = .050,889,015,196;$$

$$H = \frac{169 G}{196} = .043,878,793,714;$$

$$I = \frac{225 H}{256} = .038,565,346,037;$$

$$K = \frac{289 I}{324} = .034,399,336,434;$$

$$L = \frac{361 K}{400} = .031,045,401,131;$$

$$M = \frac{441 L}{484} = .028,287,235,328.$$

But when v is $= s$, as it is in the cafe of an arch of 90°,

$\dfrac{vv}{ss}$ and all its powers will be $= 1$, and the twelve terms

$$A - \frac{B vv}{ss} + \frac{C v^4}{s^4} - \frac{D v^6}{s^6} + \frac{E v^8}{s^8} - \frac{F v^{10}}{s^{10}} + \frac{G v^{12}}{s^{12}} - \frac{H v^{14}}{s^{14}} + \frac{I v^{16}}{s^{16}} - \frac{K v^{18}}{s^{18}} + \frac{L v^{20}}{s^{20}} - \frac{M v^{22}}{s^{22}}$$

will

will be equal to their co-efficients A−B+C−D+E−F+G−H+I−K+L−M. Therefore in this cafe the firſt twelve terms of this ſeries are

$$1.000,000,000,000, -.250,000,000,000,$$
$$+ .140,625,000,000, -.097,656,250,000,$$
$$+ .074,768,066,406, -.060,562,133,788,$$
$$+ .050,889,015,196, -.043,878,793,714,$$
$$+ .038,565,346,037, -.034,399,336,434,$$
$$+ .031,045,401,131, -.028,287,235,328,$$

which are $= 1.335,892,828,770, -.514,783,749,264,$
$$= .821,109,079,506.$$

Art. 20. The remaining part of this ſeries is

$$\frac{N v^{24}}{s^{24}} - \frac{O v^{26}}{s^{20}} + \frac{P v^{28}}{s^{28}} - \frac{Q v^{30}}{s^{30}} + \frac{R v^{32}}{s^{32}} - \frac{S v^{34}}{s^{34}} + \frac{T v^{36}}{s^{36}} - \frac{V v^{38}}{s^{38}} + \&c.$$

or $\dfrac{23.23.M v^{24}}{24.24.s^{24}} - \dfrac{25.25.N v^{26}}{26.26.s^{26}} + \dfrac{27.27.O v^{28}}{28.28.s^{28}} - \dfrac{29.29.P v^{30}}{30.30.s^{30}} + \dfrac{31.31.Q v^{32}}{32.32.s^{32}}$

$$- \frac{33.33.R v^{34}}{34.34.s^{34}} + \frac{35.35.S v^{36}}{36.36.s^{36}} - \frac{37.37.T v^{38}}{38.38.s^{38}} + \&c. \text{ or}$$

$$.025,979,075,500, \times \frac{v^{24}}{s^{24}} - .024,019,115,661 \times \frac{v^{26}}{s^{20}}$$

$$+ .022,334,101,169, \times \frac{v^{28}}{s^{28}} - .020,869,976,759 \times \frac{v^{30}}{s^{30}}$$

$$+ .019,585,984,048, \times \frac{v^{32}}{s^{32}} - .018,450,810,232, \times \frac{v^{34}}{s^{34}}$$

$$+ .017,440,001,955, \times \frac{v^{36}}{s^{36}} - .016,534,184,679, \times \frac{v^{38}}{s^{38}}$$

$+ \&c.;$ which is $= \frac{v^{24}}{s^{24}} \times$ the ſeries

$$.025,979,075,500, - .024,019,115,661, \frac{vv}{ss}$$

$$+ .022,$$

$+ .022,334,101,169, \dfrac{v^4}{s^4} - .020,869,976,759, \dfrac{v^6}{s^6}$

$+ .019,585,984,048, \dfrac{v^8}{s^8} - .018,450,810,232, \dfrac{v^{10}}{s^{10}}$

$+ .017,440,001,955, \dfrac{v^{12}}{s^{12}} - .016,534,184,679, \dfrac{v^{14}}{s^{14}}$

$+$ &c. or, if we fubftitute x in this laft feries inftead of $\dfrac{vv}{ss}$, $= \dfrac{v^{24}}{s^{24}} \times$ the feries

$.025,979,075,500, - .024,019,115,661, x$

$+ .022,334,101,169, xx - .020,869,976,759, x^3$

$+ .019,585,984,048, x^4 - .018,450,810,232, x^5$

$+ .017,440,001,955, x^6 - .016,534,184,679, x^7$

$+$ &c. Now the value of this laft feries may be difcovered by the application of the differential feries

$$a - \frac{bx}{1+x} - \frac{\mathrm{D^I} xx}{\overline{1+x}|^2} - \frac{\mathrm{D^{II}} x^3}{\overline{1+x}|^3} - \frac{\mathrm{D^{III}} x^4}{\overline{1+x}|^4} - \frac{\mathrm{D^{IV}} x^5}{\overline{1+x}|^5} - \frac{\mathrm{D^{V}} x^6}{\overline{1+x}|^6} - \frac{\mathrm{D^{VI}} x^7}{\overline{1+x}|^7} - \&c.$$

in the manner following:

Here a is $= .025,979,075,500;$

$\quad\quad b \quad = .024,019,115,661;$

$\quad\quad c \quad = .022,334,101,169;$

$\quad\quad d \quad = .020,869,976,759;$

$\quad\quad e \quad = .019,585,984,048;$

$\quad\quad f \quad = .018,450,810,232;$

$\quad\quad g \quad = .017,440,001,955;$

and $\quad b \quad = .016,534,184,679.$

Therefore

Therefore the differences of b, c, d, e, f, g, and h, of the several succeffive orders, are as follows:

First differences.

$b - c$ is $= .001,685,014,492$;
$c - d$ $= .001,464,124,410$;
$d - e$ $= .001,283,992,711$;
$e - f$ $= .001,135,173,816$;
$f - g$ $= .001,010,808,277$;
$g - h$ $= .000,905,817,276$.

Second differences.

$.000,220,890,082$;
$.000,180,131,699$;
$.000,148,818,895$;
$.000,124,365,539$;
$.000,104,991,001$.

Third differences.

$.000,040,758,383$;
$.000,031,312,804$;
$.000,024,453,356$;
$.000,019,374,538$.

Fourth differences.

$.000,009,445,579$;
$.000,006,859,448$;
$.000,005,078,818$.

Fifth differences.

$.000,002,586,131$;
$.000,001,780,630$.

Sixth differences.

$.000,000,805,501$.

Therefore D^{I} is $= .001,685,014,492$;

$\quad\quad\quad D^{II} \quad = .000,220,890,082$;

$\quad\quad\quad D^{III} \quad = .000,040,758,383$;

$\quad\quad\quad D^{IV} \quad = .000,009,445,579$;

$\quad\quad\quad D^{V} \quad = .000,002,586,131$;

$\quad\quad\quad D^{VI} \quad = .000,000,805,501$.

Confequently the differential feries

$$a - \frac{bx}{1+x} - \frac{D^{I}xx}{\overline{1+x}|^{2}} - \frac{D^{II}x^{3}}{\overline{1+x}|^{3}} - \frac{D^{III}x^{4}}{\overline{1+x}|^{4}} - \frac{D^{IV}x^{5}}{\overline{1+x}|^{5}} - \frac{D^{V}x^{6}}{\overline{1+x}|^{6}} - \frac{D^{VI}x^{7}}{\overline{1+x}|^{7}} - \&c. \text{ is} = \text{to}$$

$.025,$

$$.025,979,075,500,$$

$$- .024,019,115,661, \times \frac{x}{1+x}$$

$$- .001,685,014,492, \times \frac{xx}{\overline{1+x}|^2}$$

$$- .000,220,890,082, \times \frac{x^3}{\overline{1+x}|^3}$$

$$- .000,040,758,383, \times \frac{x^4}{\overline{1+x}|^4}$$

$$- .000,009,445,579, \times \frac{x^5}{\overline{1+x}|^5}$$

$$- .000,002,586,131, \times \frac{x^6}{\overline{1+x}|^6}$$

$$- .000,000,805,501, \times \frac{x^7}{\overline{1+x}|^7}$$

$$- \&c.$$

This is the general value of the faid differential feries, whatever may be the value of x, or $\frac{vv}{ss}$. But in the cafe here fuppofed of an arch of $90°$, the verfed fine v is equal to the fine s; and therefore $\frac{vv}{ss}$, or x, is $= 1$, and $\frac{x}{1+x} = \frac{1}{1+1}$, or $\frac{1}{2}$. Therefore the foregoing differential feries is in this cafe equal to

$$.025,979,075,500,$$

$$- .024,019,115,661, \times \frac{1}{2}$$

$$- .001,685,014,492, \times \frac{1}{4}$$

$$- .000;$$

$$- .000,220,890,082, \times \frac{1}{8}$$

$$- .000,040,758,383, \times \frac{1}{16}$$

$$- .000,009,445,579, \times \frac{1}{32}$$

$$- .000,002,586,131, \times \frac{1}{64}$$

$$- .000,000,805,501, \times \frac{1}{128}$$

$$- \&c. = .025,979,075,500, - .012,009,557,830,$$
$$- .000,421,253,623,$$
$$- .000,027,611,260,$$
$$- .000,002,547,398,$$
$$- .000,000,295,174,$$
$$- .000,000,040,408,$$
$$- .000,000,006,292,$$
$$- \&c.$$

$$= .025,979,075,500, - .012,461,311,985, \&c.$$
$$= .013,517,763,515, - \&c.$$ Therefore the series

$$a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 + \&c., \text{ or}$$

$$.025,979,075,500 - .024,019,115,661, x$$

$$+ .022,334,101,169, xx - .020,869,976,759, x^3$$

$$+ .019,585,984,048, x^4 - .018,450,810,232, x^5$$

$$+ .017,440,001,955, x^6 - .016,534,184,679, x^7$$

$+$ &c. is in this case $= .013,517,763,515, - \&c.$
Therefore $\frac{v^{24}}{s^{24}} \times$ this last series, or $\frac{v^{24}}{s^{24}} \times$ the series

.025,979,075,500, − .024,019,115,661, $\frac{v^8}{s^8}$

$\div$ .022,334,101,169, $\frac{v^4}{s^4}$ − .020,869,976,759, $\frac{v^5}{s^8}$

$\div$ .019,585,984,048, $\frac{v^8}{s^8}$ − .018,450,810,232, $\frac{v^{10}}{s^{10}}$

$\div$ .017,440,001,955, $\frac{v^{12}}{s^{12}}$ − .016,534,184,679, $\frac{v^{14}}{s^{14}}$

$\div$ &c. is in this case $= \frac{v^{24}}{s^{24}} \times$ .013,517,763,515, − &c.; that is, the series

.025,979,075,500, $\frac{v^{24}}{s^{24}}$ − .024,019,115,661, $\frac{v^{26}}{s^{26}}$

$\div$ .022,334,101,169, $\frac{v^{28}}{s^{28}}$ − .020,869,976,759, $\frac{v^{30}}{s^{30}}$

$\div$ .019,585,984,048, $\frac{v^{32}}{s^{32}}$ − .018,450,810,232, $\times \frac{v^{34}}{s^{34}}$

$\div$ .017,440,001,955, $\frac{v^{36}}{s^{36}}$ − .016,534,184,679, $\frac{v^{38}}{s^{38}}$

$\div$ &c., or $\frac{N v^{24}}{s^{24}} - \frac{O v^{26}}{s^{26}} + \frac{P v^{28}}{s^{28}} - \frac{Q v^{30}}{s^{30}} + \frac{R v^{32}}{s^{32}} - \frac{S v^{34}}{s^{34}} + \frac{T v^{36}}{s^{36}} - \frac{V v^{38}}{s^{38}} +$ &c. is

in this case $= \frac{v^{24}}{s^{24}} \times$ .013,517,763,515, − &c. = (because

v is in this case $= s$, and consequently $\frac{v^{24}}{s^{24}}$ is $= 1$)

.013,517,763,515, − &c. But we before found the value of the first twelve terms of the series

$A - \frac{B v v}{s s} + \frac{C v^4}{s^4} - \frac{D v^6}{s^6} + \frac{E v^8}{s^8} - \frac{F v^{10}}{s^{10}} +$ &c. to be in this case =

.821,109,079,506. Therefore the value of the whole

series $A - \frac{B v v}{s s} + \frac{C v^4}{s^4} - \frac{D v^6}{s^6} + \frac{E v^8}{s^8} - \frac{F v^{10}}{s^{10}} +$ &c. *ad infinitum* is in

this case = .821,109,079,506, + .013,517,763,515,

-- &c. $= 834,626,843,021, -$ &c. of which the firſt eight figures .834,626,84 are exact, the error being in the ninth figure 3, which ought to be a 2 inſtead of a 3, as would have appeared if we had computed another term or two of the differential ſeries.

Art. 21. Since the ſeries $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}}$ + &c., or $I - \frac{1.1.Avv}{2.2.ss} + \frac{3.3.Bv^4}{4.4.s^4} - \frac{5.5.Cv^6}{6.6.s^6} + \frac{7.7.Dv^8}{8.8.s^8} - \frac{9.9.Ev^{10}}{10.10.s^{10}} -$ &c. is, in this caſe of an arch of $90°$, $= .834,626,843, -$ &c., or ſomewhat leſs than .834,626,843, the product of that ſeries into $\pi \times \frac{rv}{s}$, will be $= \pi \times \frac{rv}{s} \times .834,626,843, -$ &c. $=$ (becauſe v is in this caſe $= s$) $\pi \times r \times .834,626,843, -$ &c.

$= 1.570,796,326,794,$ &c. $\times r \times .834,626,843$ $-$ &c. $= r \times 1.311,028,779, -$ &c., or ſomething leſs than $r \times 1.311,028,779$; which is exact to nine places of figures, the more exact value of this quantity being $r \times 1.311,028,777,146,$ &c. as appears by a computation made by Mr. STIRLING, in his admirable Treatiſe on the Summation of Serieſes, p. 58.

Art. 22. This value of the product of $1.570,796,$ $326,794,$ &c. $\times \frac{rv}{s}$ into the ſeries

$I - \frac{1.1 Avv}{2.2.ss} + \frac{3.3.Bv^4}{4.4.s^4} - \frac{5.5.Cv^6}{6.6.s^6} + \frac{7.7 Dv^8}{8.8.s^8} - \frac{9.9.rv^{10}}{10.10.s^{10}} +$ &c., found by the foregoing proceſſes in this extreme and moſt difficult caſe, to wit, $1.311,028,779,$ &c. $\times r$, exceeds its true

value,

value, 1.311,028,777,146, &c. × *r* by only .000,000, 002, × *r*, or two thousand-millionth parts of the radius ; which is indeed a most minute difference, and shews the great exactness and utility of this differential series.

Art. 23. Of the nine figures to which the number 1.311,028,779, found by the foregoing process, is exact, the last eight are owing to the differential series. For if we were to multiply the value of the first twelve terms only of the series $A - \frac{B v v}{s s} + \frac{C v^4}{s^4} - \frac{D v^6}{s^6} + \frac{E v^8}{s^8} - \frac{F v^{10}}{s^{10}} + $ &c, or

$$1 - \frac{1 \cdot 1 . A v v}{2.2.s s} + \frac{3.3.B v^4}{4 \cdot 4.s^4} - \frac{5.5.C v^6}{6.6.s^6} + \frac{7.7.D v^8}{8.8.s^8} - \frac{9.9 \cdot E v^{10}}{10.10.s^{10}} + $$ &c., to wit, the

number .821,109,079,506, into $\pi \times r$, or 1.570,796, 326,794, &c. × *r*, the product would be only 1.289, &c. × *r*, which is true to only one place of figures, the second figure being a 2 instead of a 3. This therefore is an eminent proof of the utility of the said differential series.

Art. 24. In an arch of 90° the versed sine is equal to the radius of the circle, that is, according to the foregoing notation, *v* is = *r*. Therefore by art. 17. together with the foregoing computation, it appears, that the time of the descent of a pendulum, or other heavy body (moving freely from a state of rest by the force of gravity only) through the arch of a whole quadrant of a circle is to the time of the fall through the cor-

respondent

respondent perpendicular altitude, or the radius, as 1.311,028,779, – &c. × *r* is to *r*, or as 1.311,028,779, – &c. is to 1.

Art. 25. Hence we may determine the proportion of the time of defcent of a pendulum through an arch of 90° to the time of its defcent through an infinitely fmall arch at the bottom of a quadrant, or rather (to fpeak correctly) to the limit of the time of defcent through a very fmall but finite arch at the bottom of the quadrant, to which the faid time continually approaches nearer and nearer as the faid fmall arch is taken lefs and lefs, and to which it may be made to approach fo nearly, by taking the faid fmall arch fufficiently fmall, as to differ from it by lefs than any given quantity. For this latter time, or limit, is known to be to the time of the fall of a heavy body through half the length of the pendulum, or half the radius of the circle, as the femi-circumference of a circle is to its diameter, that is, as the number 1.570,796,326,794, &c. is to 1. But the time of the fall of a heavy body through half the radius of the circle is to the time of the fall through the whole radius as 1 to $\sqrt{2}$, or 1.414,213, &c. Therefore, *ex æquo*, the faid limit of the time of defcent of a pendulum through a very fmall arch of the circle at the bottom of the qua-
drant, is to the time of the fall of a heavy body through

the

the radius of the circle, or the whole length of the pen-
dulum, as 1.570,796, &c. is to 1.414,213, &c. But
we have seen in the last article that the time of the fall
of a heavy body through the radius of the circle is to
the time of descent of a pendulum through the arch of
a whole quadrant as 1 to 1.311,028,779, – &c. There-
fore the limit of the time of descent of a pendulum
through a very small arch at the bottom of the quadrant
is to the time of descent through the arch of the whole
quadrant as 1.570,796,326, &c. × 1 is to 1.414,213,
&c. × 1.311,028,779, – &c., or as 1.570,796,326, &c.
is to 1.414,213, &c. × 1.311,028,779, – &c., that is,
by art. 21. as 1.570,796,326, &c. is to 1.414,213, &c.
× 1.570,796,326, &c. × .834,626,843, – &c., or as
1 to 1.414,213, &c. × .834,626,843, – &c., or as 1 to
1.180,340, &c., or, in smaller numbers, as 1 to 1.180,
or as 1000 to 1180, or as 100 to 118, or as 50 to 59.

Art. 26. This proportion of the times of the de-
scent of a pendulum through an infinitely small arch
at the bottom of the quadrant, and through the arch
of the whole quadrant, agrees pretty nearly with
that assigned for them by Mr. HUYGENS in the pre-
face to his admirable Treatise on Pendulum-clocks,
or *De Horologio Oscillatorio*, which is that of 29 to
34. For 50 is to 59 as 29 is to 34.2, or $34\frac{1}{5}$; or
(neglecting the fraction $\frac{1}{5}$) as 29 is to 34; Mr. HUYGENS

meaning

meaning, probably, in that place, not to exprefs this proportion as accurately as he could, but only as nearly as it could be expreffed by fmall whole numbers. However, the numbers 50 and 59 exprefs this proportion rather more accurately than 29 and 34, and with pretty much the fame degree of fimplicity, and therefore, upon the whole, are fomewhat to be preferred to them.

Art. 27. I have endeavoured to find another differential feries, fimilar to that above defcribed, for the purpofe of inveftigating the value of an infinite feries of this form, to wit, $a + bx + cxx + dx^3 + ex^4 + fx^5 + gx^6 + hx^7 + \&c.$ (in which all the terms are marked with the fign +, or are added to the firft term a) when the co-efficients b, c, d, e, f, g, h, &c. decreafe very flowly, and x is very nearly equal to 1, and the terms of the feries decreafe confequently fo flowly as to make the fummation of it in the common way, or by the mere computation and addition of its terms, almoft impracticable; but my endeavours have not been attended with fuccefs. I may therefore, from my own experience, fubfcribe to the truth of what is afferted upon this fubject by the very learned and ingenious Mr. JAMES STIRLING in his Treatife, intitled, *Summatio Serierum*, p. 17. to wit, that *Series quarum termini funt per vices negativi et affirmativi, funt magis tractabiles quam alteræ, ubi de Summatione agitur*; though at firft fight one would be apt to imagine the reverfe of this propofition to be true.

XI. *Tranſlation of a Paſſage in* Ebn Younes; *with ſome Remarks thereon: in a Letter from the Rev.* George Coſtard, *M. A. Vicar of* Twickenham, *to the Rev.* Samuel Horſley, *LL.D. Sec. R. S.*

REV. SIR,

HAVING, by means of the Royal Society, been favoured with a tranſcript of the Arabic paſſage in manuſcript of EBN YOUNES, in the library at Leyden, I now ſend you as exact a tranſlation of it as I can. I give it you in Latin, as the former tranſlations of it were in that language; and as the numbers in the manuſcript by no means agree with calculations made by modern tables, I have ventured to ſuppoſe that they have been ſomehow or other altered from what they were in the original tables of EBN YOUNES. I have likewiſe ventured to ſuppoſe that the preſent Leyden copy is a tranſcript of another copy, which is no very violent ſuppoſition, conſidering how long ago theſe obſervations have been made, and how long it is ſince EBN YOUNES wrote.

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I have likewiſe made no ſcruple to ſuppoſe that, however diſtinct and elegant both the Arabic letters and figures are in later manuſcripts, they were not ſo in thoſe of a more ancient date, ſo that the one might eaſily be miſtaken for the other, where there is a ſimilarity: and this miſtake would be the more eaſily committed by a perſon ignorant of the ſubject he was upon. This probably was the caſe of all ſuch as were hired by bookſellers to tranſcribe manuſcripts for ſale; and for this reaſon, when the tranſcriber had made any miſtake, he would not blot it out for fear of ſpoiling the ſale of his book.

There is an inſtance of this ſort in this very manuſcript in the obſervations of the third eclipſe, which is that of the Moon, as you will ſee in the tranſcript and tranſlation ſent you laſt year by Mr. SCHULTENS.

If what hath been ſaid be allowed me, as I hope it will not be thought too much, I think I ſhall be able to account poſſibly, if not probably, for the differences between the obſervations as ſet down in the manuſcript, and the reſult of the calculations by modern tables: a thing which hath not been hitherto attempted, as few who have been verſed in aſtronomy have been acquainted with the Arabic language; and they on the other hand, who have well underſtood Arabic, have been as little converſant with aſtronomy.

What

What I have now advanced ſhall be exemplified under the firſt eclipſe, which is one of the Sun.

In this eclipſe, according to the manuſcript, at the beginning, the Sun's altitude was more than 15 (يه) degrees, and leſs than 16 (يو); and at the end it was more than 33 degrees (لج) and $\frac{1}{3}$. But I make the Sun's height at the beginning 30 (ل) degrees, and at the end nearly 36 (لو). In the manuſcript, the digits eclipſed are ſaid to have been 8 (v, or ʌ, as it is ſometimes written); but I make them only a little more than 4(٥), or about $4\frac{4}{5}$.

Whether the notation in the original manuſcript of EBN YOUNES was in letters or arithmetical figures is uncertain; but moſt probably it was in the former of theſe two, as it is in moſt of the tables now extant, though compoſed ſince the admiſſion and uſe of arithmetical figures. Upon this ſuppoſition then, or that they were ſo in the manuſcript from whence the preſent manuſcript was copied, we ſhall very naturally account for the miſtakes we find in it.

Thus for inſtance, ل by ſome accidental ſtroke at the bottom, would eaſily be taken for لو, as لو is ſometimes written in manuſcripts; and if the perpendicular ſtroke in the ل was made ſhort, as in a table it very well might be, ل (30) would naturally be taken for لو or يه (15); and,

by the ſame rule, ‏ڋ‎ (36) would very eaſily be taken for ‏ڛ‎ (16); and ‏ۮ‎ (4) the digits eclipſed for v which is 8 in the other form of notation, or ‏ﺴ‎ in this.

In the manuſcript it is ſaid, that the Sun's altitude at the end, by obſervation, was a little more than 33 (‏ﻊﻟ‎) degrees; but this would, in a manuſcript ill writen, eaſily be miſtaken for ‏ﻟ‎ (35) or ‏ڋ‎ (36).

As to the words, tranſlated by Profeſſor SCHULTENS for Mr. GRISCHOW, " accidit hoc in plano circuli ejus " minus quam 7 digiti," I am apt to ſuſpect they are nothing more than ſome marginal reading crept into the text; that is, ſomebody ſeeing the digits eclipſed here made 8 (‏ﺴ‎), added, as the Arabic will very well bear, " imo minus quam ‏ﺟ‎ (7) or v (7)," as in the other form of notation that figure is ſometimes made. The writer of this manuſcript, whoever he was, was certainly acquainted with both forms of notation, as he hath made uſe of both.

This interpretation is at leaſt plauſible, and clears up a ſentence which greatly perplexed both Mr. GRISCHOW and Dr. BEVIS, and ſeemed to them quite unintelligible.

The account given by CURTIUS of the ſecond eclipſe, which was a ſolar one, is this:

<div align="center">5</div>

<div align="right">Anno</div>

Anno eodem, die Sabbati, videlicet, 29 menſis Sywal, (numero decimi, qui Paſchalis eſt eorum) eclipſis Solis occupavit digitos 7½. In principio, Sol altus ferè 56°. In fine, Sol occiduus elevabatur gradibus 26. Ex SHICKARDO in MS.

This it is plain is not a tranſlation of the Arabic, for that, as tranſlated by SCHULTENS for Mr. GRISCHOW, and tranſmitted by him to Dr. BEVIS, is much fuller, and is as follows:

ECLIPSIS SOLARIS.

Hæc eclipſis extitit die Sabbati, 29 menſis Siewal, anno 367 Hegiræ. Et dies Sabbati hicce ipſe eſt dies 9 menſis Chordadma, anni 348 Jeſdagirdis, et ipſe 8 menſis Haziran anni 1289 Alexandri, et ipſe eſt 14 menſis Buna, anni Dioclefiani.

Fuitque maximum quod eclipſatum eſt de diametro Solis, 5 digiti et ½ ſuper calculo accuratiore.

Erantque de plano circuli ejus 4 digiti et 10 minuta.

Et erat elevatio Solis, tempore quo eclipſis incepit, ſecundum oculum 56° circiter; et erat integra ejus reapparitio cum eſſet elevatio ejus 26 graduum, aut circiter; erantque Sol et Luna ſimul in hac eclipſi, in pro-

pinquo

pinquo diſtantiæ maximæ a terrâ. Deus ſcit an calculus hic benè ſit poſitus. Tempus reſpondet diei 8 Jun. an. Chriſti 978.

Thus far Mr. SCHULTENS. And here I muſt obſerve that, according to him as well as CURTIUS, the Sun's altitude at the beginning was about 56°, or in Arabic notation نو ; but by computation I make it only about 47° 50′. Suppoſe it were 47 (مز); then where the letters are ſmall and ill made, نو and مز may eaſily be miſtaken for each other.

The Sun's altitude at the end of this eclipſe, according to both CURTIUS and SCHULTENS, was 26° (كو) ; but by calculation I make it a little more than 36° (لو). But theſe figures are ſo nearly alike that they would eaſily be miſtaken by an ignorant tranſcriber, and from a manuſcript that was ill wrote.

How SCHICKARD, or CURTIUS for him, came to make the digits eclipſed $7\frac{1}{2}$ I know not: for in the manuſcript, as tranſlated by SCHULTENS for Mr. GRISCHOW above, we ſee they were only $5\frac{1}{2}$ and that *ſuper calculo accuratiore*, or as the Arabic ſhould have been tranſlated, *juxta calculum accuratiorem.* The meaning of which, I ſuppoſe, is that EBN YOUNES had found by calculation that the digits eclipſed would be $5\frac{1}{2}$, and that at the time his calculation agreed with his obſervation; as indeed it did,

for

for I make them about 5⅗, however widely this differs from 7½ as in CURTIUS.

When the altitude of the Sun, at the beginning of this eclipſe, is ſaid to have been 56° or nearly, *ſecundum oculum*, it is evident that this was an obſervation.

When it is added, *erantque de plano circuli ejus 4 digiti et* 10 *minuta*, in words at length, it ſeems to have been ſome interpolation or marginal reading, crept into the text, as another ſeems to have done under the former eclipſe; for if the digits eclipſed here were 5½, agreeable both to obſervation and *accurate calculation*, they muſt certainly have been more than 4° 10′.

At the concluſion of the former eclipſe it was added in the tranſlation, Deus ſcit an *obſervatio* ſit benè inſtituta; and here the paſſage, as tranſlated, concludes with Deus ſcit an *calculus* hic benè ſit poſitus. But in the Arabic, as I have received it, there is no mention made either of *obſervation* or *calculation*. The words are the ſame in both paſſages, and are only *adjuvante Deo*. The other tranſlations ſeem only to have been what Mr. GRISCHOW collected from profeſſor SCHULTENS, who, he ſays, was totally ignorant of aſtronomical language, as he himſelf was ignorant of Arabic.

The third is a Lunar eclipſe; and the account given of it by CURTIUS, from SCHICKARD, is this:

Anno

Anno Chriſti 979. Anno Hegiræ 368 (qui incepit d. 8 Aug. mihi die 9 Aug. anno Chriſtiano 978) die Jovis, 14 Sywal, Luna fuit orta cum defectu, qui ad $5\frac{1}{2}$ digitos accrevit, cum extaret ſupra horizontem gradibus etiam 26 (ſubaudio finem tunc accidiſſe). SCHICKARDUS. Qui adjungit, tempus reſpondere diei 14 Maii, anno Chriſti 979.

The account of this eclipſe, as tranſlated by Profeſſor SCHULTENS for Mr. GRISCHOW, is more particular and intelligible.

Eclipſis Lunæ extitit in menſe Sieval (five Xaval) anno 368 Hegiræ. Orta eſt Luna eclipſata, in nocte cujus aurora fuit feria quinta. Et hæc feria quinta fuit dies 25 menſis Ijar, anni 1290 Alexandri, et ille 20 menſis Baſchner (five Pachon) anni 695 Diocleſiani.

Spatium quod eclipſatum fuit de diametro ejus, fuit amplius quam octo digiti, et minus quam novem.

Fuitque hora ortûs ejus proxima horæ oppoſitionis, ſecundum fundamenta quibus computare ſoleo.

Et perfecta eſt ejus reapparitio (five finis) cum præteriiſſet de nocte (i. e. poſt occaſum Solis) circiter hora juſta, et quinta horæ pars, prout obſervavi. Et erat Luna, in hac eclipſi, in propinquo diſtantiæ ſuæ mediæ.

Tempus reſpondet diei 14 Maii, anno Chriſti 979.

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With regard to the time of the oppoſition, and the Moon's riſing at Cairo, there is very little difficulty; for ſhe roſe there at 6ʰ 48′ 10″, and the time of oppoſition, was at 6ʰ 24′ 36″.

The end of this eclipſe there was at 7ʰ 54′ 26″, and the time of Sun-ſet was at 6ʰ 47′ 52″. The difference is 1ʰ 6′ 16″, and agrees very well with the manuſcript.

The paſſage, as we have it here in CURTIUS from SCHICKARD, is very obſcure. For it ſeems either to mean that when the digits eclipſed were 5½ the Moon was 26° high, or that ſhe was 26° high when the eclipſe ended. But I take the laſt to be intended; for the Moon was 26° high at 7ʰ 36′, and the eclipſe ended, as we ſaw, at 7ʰ 54′ 26″.

But when SCHICKARD or CURTIUS ſay this *defectus ad 5½ digitos accrevit*, the meaning muſt be that they amounted only to 5½. But this is not true; for according to the manuſcript, they were between 8 and 9, and I make them about 8⅗.

I am apt to ſuſpect, therefore, that the tranſcriber, whoever he was, caſt his eyes on the Solar eclipſe above, where the digits eclipſed are really 5½, and careleſsly ſet them down to this Lunar eclipſe where they do not belong. And to confirm this conjecture it muſt be obſerved, that after the word *Dioclefian* under this Lunar eclipſe, in the Arabic follow *ſix lines*, which are a repe-

tition,

tition of all that was faid under the laft folar eclipfe, from the fame word *Dioclefian* to the end of that obfervation.

I fhall now, in the laft place, give you a tranflation of the Arabic paffage intire, omitting however the interpolations mentioned above, which embarrafs the whole.

Infit ALI IBN ABDORRAHMAN, IBN ACHMED, IBN YOUNES, IBN ABDOL' AALI.

Imprimis, jam commemoravi eclipfes, tam Solares quam Lunares, quas obfervârunt viri docti; eruditi ii quorum nomina recenfui, quafque ad eos retuli, *incipiendo* ab auctoribus libri dicti ALMOMTAHEN, ufque ad filios Majour; quin et conjunctiones eorum cum ftellis fixis, quas obfervârunt, et quorum loca commemorârunt, et invenerunt, tempore conjunctionum eorum.

Ipfe deinde memorabo eclipfes quas obfervavi, tam Solares quam Lunares, et conjunctiones cum ftellis fixis, et quænam fuerunt formæ eorum in conjunctionibus fuis. Ut quicunque me fequantur, et indicia habere defiderent, meis utantur, quemadmodum ac ego eorum indiciis et directionibus ufus fum, qui ante me obfervârunt. Deus autem adjutor eft.

Eclipfis Solaris erat priore parte diei, feriâ quintâ, die decimo octavo menfis Rabiæ pofterioris, anno Hegiræ

367. Et hæc feria quinta erat dies decimus ſecundus menſis Adzermah, anno Yezdagerdis 346.

Caraffæ adfuimus, in templo ABI GAAFARI ACHMED IBN NASAR Africani, cœtus eruditorum, ad hanc elipſin obſervandam. E quorum numero erat HAROUN IBN MOHAMMED AL GAAFARI, et ABU ABDALLAH AL HOSEIN IBN NASAR Africanus, et ABUL' HOSEIN ALI IBN MAHARBACHT Perſa, et ABUL' ABAS ACHMED IBN ACHMED AL CHURGII, et ABU ACHMED ASSUMACHI, et ABU OMAR Scriba.

Ex his, præter alios eruditos cum reliquis obſervatoribus, nonnulli erant aſtronomicè docti.

Ipſe quoque eodem contendi, unâ cum ABUL' KASEM ABDORRAHMAN IBN HOSEIN, IBN TISAN, AL IDAS, et HOSAN IBN AL DARANI, et HAMED IBN AL HOSEIN.

Et hi omnes initium hujus eclipſeos obſervârunt, quæ, ad ſenſum meum, apparere incepit ſole plus quam gradibus 15, minus autem quam 16 elevato.

Omnes quoque præſentes opinione conſentierunt obſcurari de diametro ejus circiter 8 digitos.

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Et ſplendor ejus perfectè recuperatus eſt cum elevaretur amplius quam gradibus 33 cum tertiâ ferè parte, prout ipſe menſuravi; omnibus qui aderant conſentientibus.

VOL. LXVII.   I i   Iɴ

In hac eclipſi, Sol et Luna ſimul erant non longè a
diſtantiâ ſuâ proximâ a terrâ.   Adjuvante Deo.

ECLIPSIS SOLARIS.

Hæc eclipſis incidit in diem Sabbati, diem 29 menſis
Shuwal, anno Hegiræ 367.   Eratque hic dies Sabbati,
dies 9 menſis Chordadmah, anno Yezdagerdis 347, et
dies 8 menſis Haziràn, anno Alexandri 1289; et inſu-
per dies 14 menſis Bounah. anno Diocleſiani 694.

Maximum quod obſcuratum eſt de diametro ſolis erat
$5\frac{1}{2}$ digiti.

* * * * * *

Et quando hæc eclipſis, ad oculi aciem, jam incepiſſe
conſtabat, Solis altitudo erat circiter gradus 56, et lucis
ejus reſtitutio completa eſt cum altitudo ejus eſſet 26
gradus, vel circiter.

Erantque Sol et Luna ſimul, in hac eclipſi, propè diſ-
tantias ſuas maximas a terrâ.   Adjuvante Deo.

ECLIPSIS LUNARIS.

Hæc contigit menſe Shuwal, anno Hegiræ 368.   Orie-
batur Luna, eclipſi jam inchoatâ, noſte cujus Aurora
erat feria quinta, quæ feria quinta erat dies 28 menſis
Ardbaheſt, anno Yezdagerdis 348, quæ fuit 18 menſis

Ijar

Ijar, anno æræ Alexandri 1290. Eratque dies 20 menſis Biſhnis, anno Diocleſiani 698.

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Eratque quantitas diametri ejus obſcurata, pluſquam digiti 8, et minus quam novem. Tempuſque ortûs ejus erat propè tempus oppoſitionis, juxta fundamenta quibus computavi: lucemque plenam recuperavit cum de nocte præteriiſſet hora circiter æquinoctialis, cum quintâ parte, prout ipſe conjectavi.

Eratque Luna in hac eclipſi, haud procul a diſtantiâ ſuâ mediâ a terrâ. Adjuvante Deo.

This I hope will be ſufficiently ſatisfactory.

I am, &c.

XIII. *Obſervations on the Annual Evaporation at* Liverpool *in* Lancaſhire; *and on Evaporation conſidered as a Teſt of the Moiſture or Dryneſs of the Atmoſphere.* By *Dr.* Dobſon *of* Liverpool. *Communicated by John* Fothergill, *M. D. F. R. S.*

THE quantity of rain which falls during the courſe of the year, is a very uncertain teſt of the moiſture or dryneſs of any particular feaſon, fituation, or climate. There may be little or even no rain, and yet the air be conſtantly damp and foggy; or there may be heavy rains, with a comparatively dry ſtate of the atmoſphere. The ſame depth of rain will likewiſe produce different effects on the air, according as it falls upon a flat or hilly country; for large quantities ſoon quit the hills or high grounds, while ſmaller quantities have more laſting and powerful effects on a flat country. Much alſo depends upon the nature of the ſoil, whether clay or ſand, whether firm and compact, or looſe and ſpungy.

Is not evaporation therefore a more accurate teſt of the moiſture or dryneſs of the atmoſphere, than the quantity of rain?

It is well known, that air is an active solvent of water, and that its powers of solution are in proportion to its drynefs. It is likewife well known, that in chemical folutions, the action of the *menftruum* is greatly promoted by heat and agitation. If the temperature of the air then, and the ftate of the winds, be afcertained, which in the prefent cafe denote the heat and agitation of the *menftruum*, the evaporation will be the true index of the drynefs of any particular feafon, fituation, or climate.

To determine the annual evaporation in the neighbourhood of Liverpool, I procured two well-varnifhed tin veffels; one of which was to ferve the purpofe of a rain-gage; the other was to be employed as my evaporating veffel. The evaporating veffel was cylindrical, twelve inches in diameter and fix inches deep. The rain-gage confifted of a funnel twelve inches likewife in diameter, the lower end of which was received into the mouth of a large ftone-bottle; and, to prevent any evaporation from the bottle, the pipe of the funnel was ftopped with a grooved cork. Thefe veffels were placed in the middle of a grafs-plot, on a rifing ground adjoining and immediately overlooking the town, about feventy-five feet above the level of the fea, and with a free expofure to the Sun, winds, and rain. The cylindrical veffel was filled with water within two inches of the top;

top; and if, in confequence of heavy rains, there was
danger of its overflowing, a quantity of water was taken
out; but if, in confequence of long drought, it funk
lower, a quantity of water was then occafionally added;
and thefe additions or fubtractions were carefully re-
giftered. At the end of every month, the depth of rain
was firft calculated; and, as each veffel received the fame
depth of rain, I had only to examine the quantity of
water which had been added to, or taken out of, the eva-
porating veffel, and the evaporation of the month was
afcertained.

The firft column of the following tables points out
the mean temperature of the air at two in the afternoon.
The fecond, the character of the month with refpect to
the winds, the number of dots expreffing their ftrength;
and, to make this part tolerably accurate, daily obferva-
tions on the winds were marked down, and the character
of the month formed from a general furvey of thefe ob-
fervations: our winds are Wefterly for near two-thirds
of the year. The third column points out the evapora-
tion of each month in inches and decimal parts of an
inch. The fourth, the depth of rain during each month.
And the fifth, the ftate of the feafons, E being prefixed
to the evaporation of the whole three months, R to the
rain, and T to the mean temperature.   I

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It is to be obferved, that in making thefe experiments, 251 grains were allowed for every cubic inch of water; and that three pounds and twelve ounces of water give a depth of one inch on a circular area of twelve inches diameter.

TABLE I.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1772.

Months.	Temp.	Winds.	Evaporat.	Rain.	Seafons.
January	38	. . .	1.27	3.26	E. 4.87
February	39	. . . .	1.25	2.35	R. 7.23
March	44	. . .	2.35	1.62	T. 40.
April	48	. .	2.53	1.85	E. 11.40
May	57	.	4.25	3.42	R. 8.39
June	67	. .	4 62	3.12	T. 57.
July	70	. .	5.53	1.59	E. 13.20
Auguft	68	. .	5.35	3.65	R. 11.29
September	62	.	2.32	6.05	T. 66.
October	60	. .	3.18	3.42	E. 6.46
November	50	. . .	2.15	4.85	R. 10.48
December	44	. .	1.13	2.21	T. 51.
	54		35.95	37.39	

TABLE II.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1773.

	Temp.	Winds.	Evaporat.	Rain.	Seasons.
January	44	. . . .	1.85	3.15	} E. 5.74
February	42½	. . .	1.13	2.37	} R. 6.17
March	50	. . .	2.76	0.65	T. 45.
April	54	. . .	2 89	2.47	E. 9.34
May	57	. .	3.79	4.56	} R. 8 35
June	64½	.	2.66	1.42	T. 58.
July	67	. .	4.92	1.32	E. 14.02
August	70	.	5.75	2.21	} R. 10.08
September	60	. .	3.35	6.55	T. 65.
October	55	. . .	2.79	4.57	E. 5.49
November	47½	. . .	1.15	6.69	} R. 15.58
December	41½	. . .	1.55	4.32	T. 48.
	54½		34.59	40.18	

TABLE III.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1774.

	Temp.	Winds.	Evaporat.	Rain.	Seasons.
January	37	. . .	1.38	4.43	E. 5 92
February	$45\frac{1}{3}$	. . . .	1.67	2.42	R. 8.23
March	$49\frac{2}{3}$	. . .	2.87	1.38	T. 44.
April	$54\frac{1}{2}$	. . . .	4.56	2 23	E. 12.39
May	$59\frac{1}{2}$	. . .	4.31	1.65	R. 7.14
June	63.	. .	3.52	3.26	T. 59.
July	$66\frac{2}{3}$	. . .	4.97	2.68	E. 13.51
August	67	. .	4.52	2.36	R. 10.56
September	$61\frac{1}{3}$	. .	4.02	5.52	T. 65.
October	57	. .	1.95	1.68	E. 4.82
November	$46\frac{1}{3}$	. . . .	1.12	2.69	R. 6.00
December	$41\frac{3}{4}$	. .	1.75	1.63	T. $48\frac{1}{3}$.
	54		36 64	31.93	

TABLE IV.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1775.

	Temp.	Winds.	Evaporat.	Rain.	Seaſons.
January	$44\frac{1}{2}$	. . .	1.51	3.21	E. 7.10
February	49	. . . .	3.02	4.62	R. 10.28
March	$48\frac{1}{2}$	. . .	2.57	2.45	T. $47\frac{1}{3}$.
April	$57\frac{5}{8}$	. . .	3.21	1.01	E. 15.09
May	61	. . .	5.02	0.85	R. 3.98
June	$70\frac{1}{2}$	. . .	6.86	2.12	T. 63.
July	$68\frac{1}{2}$	. .	5.03	5.31	E. 12.50
Auguſt	$66\frac{1}{2}$	. .	4.42	4.26	R. 12.57
September	65	. .	3.05	4 00	T. $56\frac{1}{3}$.
October	$54\frac{1}{2}$	. . .	2.12	7.01	E. 5.27
November	45	. .	1.63	3.03	R. 13.39
December	$48\frac{1}{3}$	. .	1.52	3.35	T. $49\frac{1}{3}$.
	54		39.96	40.22	

OBSERVATIONS.

1. It is evident from theſe tables, whether we attend to ſeparate months, ſeaſons, or years, that the depth of rain is a very erroneous index of the moiſture or dryneſs of the atmoſphere. On comparing the two months July and Auguſt of the year 1772, it appears that the tem-

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perature

perature of the air, the ftate of the winds, and the eva-
poration, were nearly the fame during thefe two months,
and yet the rain of Auguft was more than double that of
July. The reafon why the greater quantity of rain had
no more effect than the fmaller in adding moifture to
the atmofphere, is obvious; for on confulting my regifter
I find, that the rain of Auguft fell in heavy fhowers, and
ran off the ground before it could be evaporated; while
that of July, falling in fmall drizzling fhowers, gave
more time for its evaporation.

Again, the temperature of the air, the ftate of the
winds, and the evaporation, were nearly the fame during
the firft three months of the year 1773, with what they
were during the laft three months of that year; the ftate
of the air therefore, with refpect to moifture and dry-
nefs, muft have been the fame during thefe two feafons;
and yet the depth of rain, in one of thefe feafons, was
much more than double what it was in the other. If
we attend to whole years the fame obfervation is con-
firmed. The rain of 1775 exceeded the rain of 1774
more than eight inches; and hence it might be con-
cluded, that the atmofphere was more moift in 1775
than in 1774; the reverfe of this, however, is found to be
the fact: for there evaporated from a conftant and deter-
minate furface of water in 1775, full three inches more

than

than evaporated from the ſame ſurface of water in 1774. Conſequently the dryneſs of the atmoſphere or its power of ſolution, during the year 1775, exceeded that of 1774.

2. If we take the medium of four years obſervations it appears, that the annual evaporation at Liverpool amounts to 36.78 inches.

Dr. HALLEY obſerved at London, that water placed in a cloſe room, where neither the winds or Sun could act upon it, exhaled only eight inches during the whole year. He makes no doubt but that the free acceſs of the winds would have trebled the quantity carried away; and that this again would have been doubled by the aſſiſtance of the Sun. Dr. HALLEY, therefore, fixes the annual evaporation of London at 48 inches [a]. If this calculation be admitted, it follows, that the annual evaporation of London exceeds the annual evaporation of Liverpool 11 inches; but were the experiments to be made in London, in the ſame circumſtances with thoſe made at Liverpool, it is probable, that this would be found to be more than the real difference.

The learned CRUQUIUS obſerved at Delft in Holland, that there exhaled from water ſet in the open air, but in a calm and ſhady place, about 30 inches; and it is not to

(a) Phil. Tranſ· N° 212.

be

be doubted, fays Dr. BROWNRIGG in his very valuable work, *The Art of making common Salt*, but that double this quantity, or 60 inches, would have exhaled, had it been placed where the Sun and winds could have had their due effects [b]. In another part of this publication, Dr. BROWNRIGG fixes the evaporation of fome parts of England at 73.8 inches during the four fummer months, May, June, July, and Auguft; and the evaporation of the whole year at upwards of 140 inches [c]. Thefe are calculations, however, which do not appear to correfpond with experience; for the whole evaporation at Liverpool, inftead of 140 inches, was only 36.78 inches. The evaporation likewife of the four fummer months, on a medium of four years, inftead of 73 inches, was only 18.88 inches.

3. Dr. HALES calculates the greateft annual evaporation from the furface of the earth in England, even that from a furface of hop-ground, at 6.66 inches [d]. If we compare this with the annual evaporation from a furface of water as determined by experiment, we find, that the latter exceeds the former about 30 inches; and that the annual evaporation from a furface of water, is to the annual evaporation from the furface of the earth in this part of England, nearly as 36 to 6, or as 6 to 1.

(b) Page 185.   *(c)* P. 189.
(d) Veg. Stat. vol. I. p. 55, 56.

4. On comparing the depth of rain with the annual evaporation of this part of Lancaſhire we find, that more falls in rain than is raiſed in vapour, even though the whole were a ſurface of water; for the rain is to the evaporation as 37.43 inches to 36.78 inches: and we farther find, that the quantity exhaled from the ſurface of the earth is little more than a ſixth part of what deſcends in rain; we muſt therefore have very large ſupplies from other regions, from the ſurrounding ſea, and from the ocean of warmer climates. Hence we ſee, why our South and South-weſt winds are ſo often accompanied with rain; for as the air ſweeps along the warmer latitudes, it involves a large proportion of moiſture, which is conſtantly and copiouſly exhaling from the ocean; and this moiſture being retained in a ſtate of ſolution till it reaches the colder climates, is then either collected in clouds or immediately precipitated in rain, according to the different conditions of the atmoſphere.

Theſe foreign ſupplies, however, are uniformly reſtored to the ſources from which they were derived: for that proportion of rain which riſes not in vapour, after moiſtening and refreſhing the earth, forms ſprings, brooks, and rivers, and is thus perpetually returning to the ocean whence it was taken; ſo truly philoſophical are the words of the preacher when ſpeaking of this vaſt circulation:

circulation: " All the rivers run into the fea, yet the fea " is not full: unto the place from whence the rivers " *come,* thither they return again."

5. About a century ago, the ingenious Mr. TOWNLEY, of Townley in this county, made fome accurate obfervations on the depth of rain which fell annually in the neighbourhood of the hills which divide Lancafhire and Yorkfhire; and on taking a medium of fifteen years, he determines it to be 41.516 inches [e]. The depth of rain, therefore, at Townley exceeds the depth of rain at Liverpool about four inches. This is probably, however, lefs than the real difference; for there was a fource of error in Mr. TOWNLEY's experiments with which the world was not at that time acquainted. Mr. TOWNLEY's rain-gage was fixed full ten yards above the furface of the earth [f]; which circumftance, according to fome later obfervations, makes a very material difference in the refult of the experiment [g]. Were the obfervations to be repeated at Townley, and the rain-gage placed upon the ground, there can be no doubt but that the depth of rain would confiderably exceed 41.516 inches; for I find from a great number of experiments, made during the laft three

(e) Phil. Tranf. abridged by Lowthorp, vol. II. p. 46. ' (f) Ibid.
(g) Phil. Tranf. vol. LIX. art. 47.

years

years with two veſſels of equal dimenſions, one placed on the ground, and the other eighteen yards higher on the battlement of the hoſpital; that the quantity received in the lower veſſel exceeds that in the higher more than one-third and leſs than one-half.

6. An ingenious friend, on peruſing theſe obſervations, aſked, " Whether the fact of evaporation going on " equally well in an exhauſted receiver, was not an un " ſurmountable objection to that theory concerning eva " poration, which ſuppoſes a chemical ſolution of water " in air?" With a view to aſcertain this fact I made the following experiment.

Two china ſaucers, each containing three ounces of water, were accurately weighed. One of them was placed in the open air; the quickſilver in the thermometer ſtood during the experiment between 48° and 50°, the day tolerably clear with a moderate breeze. The other was put under the receiver of an air-pump; the air was exhauſted, and the piſtons occaſionally worked, to draw off any of the water which might be ſuppoſed to be converted into vapour. After four hours the ſaucers were again accurately weighed; that in the open air had loſt one drachm and eight grains; the weight of the other was not ſenſibly diminiſhed.

From

From this experiment it appears, that air is a chemical folvent of water, and as fuch is undoubtedly to be confidered as one caufe of the evaporation of water. Heat is another caufe of evaporation, and when raifed to a fufficient degree may produce this effect without the intervention of air, and the evaporation confequently go on copioufly in an exhaufted receiver, agreeably to the experiments of the ingenious Dr. IRVING[i].

The following obfervations are added as a farther illuftration of this fubject. Water may exift in air in three different ftates. 1. In a ftate of perfect folution. 2. In a ftate of beginning precipitation. Or, 3. Completely precipitated, and falling in drops of rain.

In the firft inftance, where the water is in a ftate of perfect folution, the air is clear, dry, heavy, and its powers of folution ftill active, though it already contains a confiderable proportion of water. In the fecond, the air becomes moift, foggy, its powers of folution are diminifhed, and it becomes lighter in proportion as its water is depofited. It is a fingular and well-attefted fact, that it never rains in the kingdom of Peru; but that during part of the year the atmofphere is conftantly obfcured with

(i) Phipps's Voyage to the North Pole, p. 211.

vapours,

vapours, and the whole country involved in what they call *Garuas*, or thick fogs [i].

It is not neceffary to point out the caufes which thus difpofe the air to depofite its diffolved water; nor to confider with what bodies air hath a ftronger affinity than with water; neither to inquire how far the electrical fluid is engaged in the procefs. It is fufficient to obferve, that fo long as thefe caufes have a general action on the air, they diminifh its power of folution, and give a damp and foggy ftate of the atmofphere; that when they operate for a confiderable proportion of the year, they produce a moift climate; and that when they more generally do not, and the air retains its moifture in a ftate of perfect folution, the climate is dry. Confequently, that the moifture or drynefs of a climate, do not fo much depend upon the abfolute quantity of water which is contained in the air, as upon the air being in a ftate of perfect or imperfect folution. During long continued fummer droughts, a very large proportion of water is diffolved in the air; notwithftanding this, the air is ftill dry, and continues to be fo as long as the water remains in a ftate of perfect folution; no fooner are the powers of folution diminifhed, than what was before a dry, now becomes a moift climate.

(i) D'Ulloa's Voyage to South America, vol. II. p. 69.

In.

In the third inftance, the diffolved water may be either flowly precipitated and fall in drizzling rain, or it may be more powerfully difcharged in brifk rain; or there may be partial and fudden precipitations from particular regions, while other parts of the atmofphere ftill retain their water in a ftate of perfect folution.   Heavy thunder-fhowers are the moft remarkable inftances of partial, fudden, and copious precipitations.

Liverpool, Nov. 20, 1776.
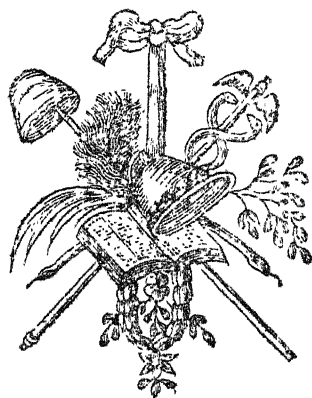
XIV. *An Account of Persons who could not distinguish Co-*
lours. By Mr. Joseph Huddart, *in a Letter to the Rev.*
Joseph Priestley, *LL.D. F. R. S.*

S I R,

Read Feb. 13, 1777. WHEN I had the pleasure of waiting on you last winter, I had hopes before now of giving you a more perfect account of the peculiarity of vision which I then mentioned to you, in a person of my acquaintance in the North: however, if I give you now the best I am able, I persuade myself you will pardon the delay.

I promised to procure you a written account from the person himself, but this I was unfortunately disappointed in, by his dying suddenly of a pleurisy a short time after my return to the country.

You will recollect I told you that this person lived at Mary-port in Cumberland, near which place, *viz.* at Allonby, I myself live, and having known him about ten years have had frequent opportunities of conversing with him.

7

His

His name was HARRIS, by trade a shoe-maker. I had often heard from others that he could discern the form and magnitude of all objects very distinctly, but could not distinguish colours. This report having excited my curiosity, I conversed with him frequently on the subject. The account he gave was this: That he had reason to believe other persons saw something in objects which he could not see; that their language seemed to mark qualities with confidence and precision, which he could only guess at with hesitation, and frequently with error. His first suspicion of this arose when he was about four years old. Having by accident found in the street a child's stocking, he carried it to a neighbouring house to inquire for the owner: he observed the people called it a *red* stocking, though he did not understand why they gave it that denomination, as he himself thought it completely described by being called *a stocking.* The circumstance, however, remained in his memory, and together with subsequent observations led him to the knowledge of his defect. As the idea of colours is among the first that enters the mind, it may perhaps seem extraordinary that he did not observe his want of it still earlier. This, however, may in some measure be accounted for from the circumstance of his family being quakers, among whom.

whom a general uniformity of colours is known to prevail.

He obferved alfo that, when young, other children could difcern cherries on a tree by fome pretended difference of colour, though he could only diftinguifh them from the leaves by their difference of fize and fhape. He obferved alfo, that by means of this difference of colour they could fee the cherries at a greater diftance than he could, though he could fee other objects at as great a diftance as they; that is, where the fight was not affifted by the colour. Large objects he could fee as well as other perfons; and even the fmaller ones if they were not enveloped in other things, as in the cafe of cherries among the leaves.

I believe he could never do more than guefs the name of any colour; yet he could diftinguifh white from black, or black from any light or bright colour. Dove or ftraw-colour he called white, and different colours he frequently called by the fame name: yet he could difcern a difference between them when placed together. In general, colours of an equal degree of brightnefs, however they might otherwife differ, he frequently confounded together. Yet a ftriped ribbon he could diftinguifh from a plain one; but he could not tell what the colours were with any tolerable exactnefs. Dark colours

in

in general he often miftook for black, but never ima-
gined white to be a dark colour, nor a dark to be a white
colour.

He was an intelligent man, and very defirous of un-
derftanding the nature of light and colours, for which
end he had attended a courfe of lectures in natural phi-
lofophy.

He had two brothers in the fame circumftances as to
fight; and two other brothers and fifters who, as well as
their parents, had nothing of this defect.

One of the firft mentioned brothers, who is now liv-
ing, is mafter of a trading veffel belonging to Mary-
port. I met with him in December 1776, at Dublin,
and took the opportunity of converfing with him. I
wifhed to try his capacity to diftinguifh the colours
in a prifm, but not having one by me, I afked him,
Whether he had ever feen a rain-bow? He replied, He
had often, and could diftinguifh the different colours;
meaning only, that it was compofed of different colours,
for he could not tell what they were.

I then procured and fhewed him a piece of ribbon:
he immediately, without any difficulty, pronounced it a
ftriped and not a plain ribbon. He then attempted to
name the different ftripes: the feveral ftripes of white
he uniformly, and without hefitation, called white: the
four black ftripes he was deceived in, for three of them
he

he thought brown, though they were exactly of the fame fhade with the other, which he properly called black. He fpoke, however, with diffidence as to all thofe ftripes; and it muft be owned, the black was not very diftinct: the light green he called yellow; but he was not very pofitive: he faid, " I think this is what you call yellow." The middle ftripe, which had a flight tinge of red, he called a fort of blue. But he was moft of all deceived by the orange colour; of this he fpoke very confidently, faying, " This is the colour of grafs; this is green." I alfo fhewed him a great variety of ribbons, the colour of which he fometimes named rightly, and fometimes as differently as poffible from the true colours.

I afked him, Whether he imagined it poffible for all the various colours he faw, to be mere difference of light and fhade; whether he thought they could be various degrees between white and black; and that all colours could be compofed of thefe two mixtures only? With fome hefitation he replied, No, he did imagine there was fome other difference.

I could not conveniently procure from this perfon an account in writing; but I have given his own words, having fet them down in writing immediately. Befides, as this converfation happened only the 10th of laft

month

months, it is still fresh in my memory. I have endeavoured to give a faithful account of this matter, and not to render it more wonderful than it really is.

It is proper to add, that the experiment of the striped ribbon was made in the day-time, and in a good light.

I am, SIR, &c.

XV. *A new Theory of the Rotatory Motion of Bodies affected by Forces disturbing such Motion.* *By Mr.* John Landen, *F. R. S.*

I AM induced to confider this paper as not unworthy the notice of this Society, through a perfuafion that the theory herein contained will conduce to the improvement of fcience, by enabling the reader to form a true idea, and accordingly to make a computation of the motion (or change) of the axis about which a body having a rotatory motion will turn, or have a tendency to turn, upon being affected by a force difturbing its rotation; particularly of the motion of the earth's axis arifing from the attraction of the Sun and Moon on the protuberant matter of the earth above its greateft infcribed fphere: which compound motion, I conceive, has not been rightly explained by any one of the eminent mathematicians whofe writings on the fame fubject have come to my hands. Whether in this effay I have really fucceeded better than other writers who have attempted an explanation of fuch motion, I fubmit to gentlemen well verfed in mechanics to determine.

1. Fig.

1. Fig. 1. Let the fphere ADBE, whofe radius is r, revolve uniformly about the diameter ACB as an axis, with the angular velocity c, meafured at D or E, the motion being according to the order of the letters DGEH in the fection at right angles to ACB, fig. 2.; and, whilft it is fo revolving, let the pole A be impelled by fome inftantaneous percuffive force to turn about the diameter DCE, from A towards H, with the velocity w. It is propofed to find the new axis about which the fphere will revolve after receiving fuch impulfe.

Calling al, parallel to DC, x; cl will be $= \sqrt{r^2 - x^2}$: the velocity of the point a (about ACB) before the impulfe on A will be $= \frac{cx}{r}$; and the velocity (about DCE) given to the fame point (a) by the faid impulfe will be $= \frac{w\sqrt{r^2 - x^2}}{r}$. Which velocities of the point a being in contrary directions, if it be fo fituated that they be equal, then, one deftroying the other, that point will ftop and become one of the new poles fought, about which the former poles A and B will revolve with the velocity w; and the points D and E will revolve with the fame velocity (c) as before the perturbating impulfe on the point A; but inftead of defcribing the great circle DGEH, their motion will be about the new axis ab; about which they (as well as the points A and B) will defcribe leffer circles parallel

to the great circle de, in which the points d and e (de being at right angles to ab) will revolve about the fame axis (ab) with the velocity $\sqrt{c^2 + w^2}$. Which being denoted by e, and m and n being put for the fine and cofine of the angle AC a to the radius 1, me will be $= w$, $ne = c$, and confequently $mne^2 = cw$.

Now taking $\frac{cx}{r} = \frac{w\sqrt{r^2 - x^2}}{r}$, in order to find that new axis ab, we have from that equation $x = \frac{rw}{\sqrt{c^2 + w^2}} = \frac{rw}{e} = al$.

Moreover it is obvious, that if a fpheroid, a cylinder, or any other body, whofe center of gravity is c and proper axis ACB, were, whilft revolving about that axis with the fame angular velocity (c), to receive fuch an impulfe as inftantly to give the point A the angular velocity w about DCE; the axis about which that fpheroid, cylinder, or other body, immediately after the impulfe, would revolve, or would have a tendency to revolve, would be the fame line ab.

The great circle de (fig. 1.) and any other great circle fo fituated with refpect to the axis of any revolving fphere, I fhall denominate the *mid-circle*.

2. In the manner above defcribed the poles of the fphere are by the inftantaneous impulfe on the point A inftantly changed from A and B to a and b. But if, inftead of fuch impulfe, a continued attractive force F (like that of gravity)

vity) acted at A fig. 3. and at the new poles $\acute{a}$, $\overset{\shortmid\shortmid}{a}$, &c. as they become such by a succeffive change caufed by fuch continued action of the force F urging the fphere at every inftant to revolve about the diameter $\overset{\shortmid\,\shortmid}{d}\overset{}{e}$, or $\overset{\shortmid\shortmid\,\shortmid\shortmid}{d}\overset{}{e}$, &c. of the contemporary mid-circle, the new pole $(\acute{a}, \overset{\shortmid\shortmid}{a},$ &c.) would not inftantly be at a finite diftance from the primitive pole A, but fome finite time would be requifite, that by fuch fucceffive change, the pole might be varied to a finite diftance from A: and the force F continuing invariable, the velocity (v) wherewith the pole changed its place would be expreffed by $\frac{z}{t}$, t being the time elapfed whilft the pole is varying from A to a, and z the length of the arc Aa. Therefore the velocity wherewith the pole will change its place during fuch action of the force F will be expreffed in the fame manner as the velocity (v) of a body moving uniformly from A to a in the time t may be expreffed; that is, in both cafes v will be $= \frac{z}{t}$. But there is a material difference between the motion of a body fo moving from A to a and the change of place of the pole $\acute{a}$, $\overset{\shortmid\shortmid}{a}$, &c. the former is permanent, and will continue to carry the body forward without the action of any force whatever; whereas the latter will inftantly ceafe, and the axis will keep its pofition, if the force

force F ceafes to act thereon; like as the varying direc-
tion of a projectile near the earth's furface would imme-
diately ceafe to change, if the force of gravity ceafed to
act.

It is obfervable, that whilft the force F acts, and the
revolving fphere, in confequence of fuch action, every
moment takes a new axis, the angular motion about the
axis will continue invariable; the action of fuch force
only altering the axis without altering the angular velo-
city of the fphere about it: like as the direction of a
moving body is altered, without altering the velocity
thereof, by an attractive force continually acting on it in
a direction at right angles to that in which the body is
moving. And if ever the force F fhall ceafe to act, the
fphere will inftantly revolve with its primitive velocity
(*c*) about the axis it then may have been brought to take
by the preaction of that force.

The new axis, about which the fphere has fuch ten-
dency to revolve at any inftant during the action of the
force F, I fhall call the *momentary axis*; and the poles
thereof the *momentary poles.*

3. From the equation $\frac{cx}{r}=\frac{w\sqrt{r^2-x^2}}{r}$ (art. 1.) we have
$\frac{w}{x}=\frac{c}{\sqrt{r^2-x^2}}$. Now if a continued attractive force (F) act
during the time *t* as above-mentioned, inftead of the

4   inftan-

inftantaneous percuffive force at A, we, according to the doctrine of fluxions, muft, inftead of w, take $\dot{w}$, or its equal $F\dot{t}$, and $\dot{x}$ inftead of x, in the expreffion $\frac{w}{x}$; therefore, in this cafe, we have $\frac{\dot{w}}{\dot{x}} = \frac{F\dot{t}}{\dot{x}} = \frac{c}{\sqrt{r^2 - x^2}}$. Whence, putting z for the arc (A$\overset{\prime}{a}$, or A$\overset{\prime\prime}{a}$, &c.) whofe fine is x, and writing $\dot{z}$ for its equal $\frac{r\dot{x}}{\sqrt{r^2 - x^2}}$, we get $\frac{rF\dot{t}}{\dot{z}} = c$, or $\dot{z} = \frac{rF\dot{t}}{c}$.

Hence v denoting the velocity wherewith the momentary pole ($\overset{\prime}{a}$, $\overset{\prime\prime}{a}$, &c.) changes its place during the action of the accelerative force F, we have $\dot{z} = v\dot{t} = \frac{rF\dot{t}}{c}$, and confequently $v = \frac{rF}{c}$.

4. The value of v may alfo be determined in the following manner (fig. 4.). Conceive a very thin ftring (without weight) to have one of its ends faftened to a fixed point l and the other to a heavy particle of matter m; alfo conceive fuch particle fo to revolve with the velocity e, about the line ln, that a certain accelerative force F (like that of gravity referred to a certain direction) continually acting on the faid particle m, in a direction at right angles both to the ftring lm and to the tangent to the curve in which m is moving, the ftring fhall defcribe a conical furface. Then lm being denoted

by

by r, and mo, perpendicular to ln, by q; $\frac{e^2}{q}$, the centri-fugal force urging m in the direction om, will be to F as r to $\sqrt{r^2-q^2}=lo$. Therefore F muſt be $=\frac{e^2\sqrt{r^2-q^2}}{rq}$. Now if, whilſt m is ſo revolving, the force F ceaſes acting, the ſaid particle (m) will, it is obvious, immediately proceed to deſcribe a great circle of the ſphere whoſe radius is r and center l, of which great circle one of the poles will be ſituated in a leſſer circle parallel to, and 90° diſtant from, that deſcribed by m during ſuch action of the ſaid force; which pole, during ſuch action, will change its place in the ſaid leſſer circle in which it will at any time be found with a velocity (v) which will be to e as (s) the radius of the laſt mentioned circle to q. But s will be $\sqrt{r^2-q^2}$; therefore we have $v:e::\sqrt{r^2-q^2}:q$, and $\frac{\sqrt{r^2-q^2}}{q}$ $=\frac{v}{e}$. Conſequently F $=\frac{e^2\sqrt{r^2-q^2}}{rq}$ will be $=\frac{e^2}{r}\times\frac{v}{e}=\frac{ev}{r}$, and $v=\frac{rF}{e}$.

Let now m be a point on the ſurface of a ſphere whoſe center is l, and radius $lm=r$; and let the ſphere revolve about an axis ſo that m ſhall deſcribe a great circle with the velocity e. If then ſuch a motive force begins to act on the ſphere, that, continuing its action, the point m ſhall always be urged by the invariable accelerative force F to move in a direction at right angles to the ray lm and

to the tangent to the curve which *m* will defcribe; that point it is obvious will, in confequence of the action of that force, defcribe a leffer circle of the fame radius (*q*) as that defcribed by the particle *m* when faftened to a ftring and acted on by the force F as above-mentioned; and the center of the fphere being always confidered as at reft, one of the momentary poles of the fphere will defcribe a circle whofe radius will be $= \sqrt{r^2 - q^2}$ parallel to, and 90° diftant from, that defcribed by the point *m*. For if the faid force were to ceafe acting, that point of the fphere would defcribe a great circle, as would the particle *m* at the ftring in the like cafe; and therefore both the faid particle and the point *m* of the fphere at every inftant having the fame tendency, and being acted on by equal accelerative forces, the effect will be the fame with refpect to the motion of each. Confequently, *v* being put to denote the velocity wherewith the momentary pole changes its place in the circle which it will defcribe whilft the motive force producing the accelerative force F acts on *m* as juft now mentioned, *v* will be $= \frac{r \text{F}}{e}$, the fame as in the preceding article, *e* here denoting that velocity which we there denoted by *c*.

5. Referring the point of action of the perturbating force to the mid-circle we have not hitherto confidered

that point as varied with a greater or lefs velocity than
(*e*) that of the point *m*; that is, with reference to fuch
circle we have always confidered the point *m* as the point
of action.   But it is obvious, that, *cæteris paribus,* the
point of action with refpect to the mid-circle (which
point we will now denote by *q)* may be varied with a
velocity greater or lefs than *e*; and that, *cæteris paribus,*
the velocity (*v*) of the momentary pole will be the fame
with what velocity foever (*q*) the point of action of the
force F be varied; the direction in which that force acts
being always at right angles to the ray (*lq*) from the
center of the fphere, and to the tangent to the curve de-
fcribed by (*q*) fuch point of action.

Yet, although *v* continues the fame whether, *cæteris
paribus,* (*u*) the velocity of the point *q* be greater, equal
to, or lefs than *e*, the immoveable circle in which the
momentary pole will be found will not continue the
fame; that circle being greater, equal to, or lefs than the
circle whofe radius is $\sqrt{r^2-q^2}$ according as *u* is lefs, equal
to, or greater than *e*, as will be made more evident by
what follows.

6. Fig. 5. Let *p* (in the great circle R *p* Q *q* T) be one
of the poles of the axis about which the fphere RSTV,
whofe radius is *r*, is revolving (according to the order of
the letters v *q* s) with the angular velocity *e*, meafured at

4   the

the diftance r from the axis; and whilft it is fo revolving let the faid pole be urged to turn about a diameter of the mid-circle v $\overset{\prime}{q}$ s towards $\overset{\prime}{q}$, by an accelerative force F, and let fuch force continue to act on the fucceffive new poles $\overset{\prime\prime}{p}$, $\overset{\prime\prime\prime}{p}$, &c. as they become fuch, always urging the fphere to turn about a diameter of the contemporary mid-circle, whilft the direction in which fuch perturbating force acts is regulated in the following manner.

Conceive the faid revolving fphere to be furrounded by an immoveable concave fphere of the fame radius r. Then the momentary pole ($\overset{\prime}{p}$, $\overset{\prime\prime}{p}$, $\overset{\prime\prime\prime}{p}$, &c.) will always be found in fome curve $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. in the faid concave fphere, and in fome curve $\overset{\prime}{p}\overset{\prime\prime}{p}\overset{\prime\prime\prime}{p}$ &c. on the revolving fphere; which laft mentioned curve will continually touch and roll along the other curve $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. on the immoveable fphere, the force F and the direction in which it acts varying in any manner whatever. Let F be invariable; then, it is obvious, the two curves fo touching each other will be circles; and if great circles $\overset{\prime}{P}\overset{\prime}{q}$, $\overset{\prime\prime}{P}\overset{\prime\prime}{q}$, $\overset{\prime\prime\prime}{P}\overset{\prime\prime\prime}{q}$, &c. be defcribed on the furface of the immoveable fphere whofe planes fhall be at right angles to the plane of the circle $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. the points $\overset{\prime}{q}$, $\overset{\prime\prime}{q}$, $\overset{\prime\prime\prime}{q}$, &c.

therein,

therein, each 90° diftant from $\overset{\prime}{P}, \overset{\prime\prime}{P}, \overset{\prime\prime\prime}{P}$, &c. refpectively, will be in a circle ($\overset{\prime}{q}\overset{\prime\prime}{q}\overset{\prime\prime\prime}{q}$ &c.) parallel to the faid circle $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. Now as a regulation to the direction in which the force F fhall urge the momentary pole, let that direction be always a tangent to the great circle fo paffing through that pole and the correfpondent point $\overset{\prime}{q}, \overset{\prime\prime}{q},$ or $\overset{\prime\prime\prime}{q}$, &c. whilft the arcs $\overset{\prime}{q}\overset{\prime\prime}{q}, \overset{\prime\prime}{q}\overset{\prime\prime\prime}{q}$, &c. are to the arcs $\overset{\prime}{P}\overset{\prime\prime}{P}, \overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$, &c. refpectively in the conftant ratio of u to v.

The direction in which the force F acts being fo regulated, it is obvious that the radius of the circle $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. being denoted by b, the radius of the circle $\overset{\prime}{q}\overset{\prime\prime}{q}\overset{\prime\prime\prime}{q}$ &c. will be $= \sqrt{r^2-b^2}$, the diftance of thefe parallel circles being 90°. Therefore their peripheries being as the velocities (v and u) with which they are defcribed, their *radii* (b and $\sqrt{r^2-b^2}$) will be in the ratio of the faid velocities; that is $v:u::b:\sqrt{r^2-b^2}$; whence, $\frac{u}{v}$ being $=\frac{\sqrt{r^2-b^2}}{b}$, b, the radius of the circle $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. is found $=\dfrac{rv}{\sqrt{u^2+v^2}}$

$=\dfrac{r}{\sqrt{1+\frac{e^2\,u^2}{r^2\,F^2}}}$; and $\sqrt{r^2-b^2}$ the radius of the circle $\overset{\prime}{q}\overset{\prime\prime}{q}\overset{\prime\prime\prime}{q}$

&c. $=\dfrac{r\,u}{\sqrt{u^2+v^2}}=\dfrac{r}{\sqrt{1+\frac{r^2\,F^2}{e^2\,u^2}}}$, v being $=\dfrac{r\,F}{e}$, the velocity where-

with

with the momentary pole $\overset{''}{p}$, $\overset{'''}{p}$, &c. changes its place.

Confequently, if $\overset{'}{\text{P}}\text{R}$ be an arc in the faid immoveable concave fphere whofe fine is $\dfrac{rv}{\sqrt{u^2+v^2}} = \dfrac{r}{\sqrt{1+\dfrac{e^2 u^2}{r^2 \text{F}^2}}}$, the great

circles $\overset{'\,'}{q}\text{P}$, $\overset{''\,''}{q}\text{P}$, $\overset{'''\,'''}{q}\text{P}$, &c. will interfect each other at the point R.

7. Moreover, the force F being invariable and acting as expreffed in the preceding article, the primitive pole $\overset{'}{p}$ and the momentary poles $\overset{''}{p}$, $\overset{'''}{p}$, &c. will all be found in a circle $\overset{'\,''\,'''}{ppp}$ &c. defcribed upon the furface of the revolving fphere, as obferved in that article; which circle, during the action of the force of F, will (as is alfo obferved in the faid article) always touch and roll along the immoveable circle ($\overset{'\,''\,'''}{\text{P P P}}$ &c.) whofe radius we have juft now found $= \dfrac{rv}{\sqrt{u^2+v^2}} = \dfrac{r}{\sqrt{1+\dfrac{e^2 u^2}{r^2 \text{F}^2}}}$; the point of contact being always the momentary pole.

Let the fine of the arc $\overset{'\,'}{\text{P Q}}$ of the great circle $\text{R}\overset{'}{p}\text{Q}\overset{'\,'}{q}\text{T}$ in the revolving fphere be equal to k, the radius of the faid circle $\overset{'\,''\,'''}{ppp}$ &c. then will the point Q and its oppo-fite point (o) in the furface of the faid fphere, during the action of the force F, defcribe circles in the fur-

3.   rounding;

rounding immoveable concave fphere parallel to ($P\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{}$

&c.) the circle defcribed by the momentary pole $\overset{\prime}{p}$, $\overset{\prime\prime}{p}$, $\overset{\prime\prime\prime}{}$

&c. in the fame concave fphere. And fuch point Q and

its oppofite point (o) being continually urged by the

force F in directions at right angles to the tangents to

the arcs they defcribe, their velocity will continue the

fame as before the action of the faid force commenced;

which velocity, and the radius of the faid circle $\overset{\prime}{p}\overset{\prime\prime}{p}\overset{\prime\prime\prime}{p}$

&c. will be determined by the following computation.

That radius being denoted by k, we have $r : k :: e : \frac{ek}{r}$,

the velocity of the point Q before the action of the force

F commenced; and $b : v :: \text{K} : \frac{\text{K}v}{b}$, the velocity of the

fame point (Q) during the action of that force, K being

put for the fine of the arc QR; therefore the velocity of

Q continuing the fame during the action of F as before,

we have $\frac{ek}{r} = \frac{\text{K}v}{b}$. But K is the fine of the fum of the arcs

$\overset{\prime}{R}\overset{\prime}{P}$, PQ, whofe fines are b and k refpectively; therefore

$\frac{b\sqrt{r^2-k^2}}{r} + \frac{k\sqrt{r^2-b^2}}{r}$ will be $= \text{K}$; and by fubftitution we get

$\frac{ek}{r} = \frac{v\sqrt{r^2-k^2}}{r} + \frac{kv\sqrt{r^2-b^2}}{r} = \frac{v\sqrt{r^2-k^2}}{r} + \frac{ku}{r}$, $\frac{\sqrt{r^2-b^2}}{b}$ being $= \frac{u}{v}$

by the preceding article. Hence we find $k =$

$$r v$$

$\dfrac{rv}{\sqrt{e-u|^2+v^2}}$; and it follows, that $\dfrac{ev}{\sqrt{e-u|^2+v^2}}$ $\left(=\dfrac{ch}{r}\right)$ will be

equal to the velocity of the point Q, and likewife of its oppofite point (o) in the furface of the fphere. It alfo follows, that K, the radius of each of the circles defcribed by thofe points, during the action of the force F will be

equal to $\dfrac{rev}{\sqrt{u^2+v^2}\times\sqrt{e-u|^2+v^2}}$,

By what is done it appears, that during the action of the force F, the motion of the revolving fphere will be

regulated by the circle $\overset{\prime\ \prime\prime\ \prime\prime\prime}{ppp}$ &c. thereon (whofe radius is

$\dfrac{rv}{\sqrt{e-u|^2+u^2}}=\dfrac{r}{\sqrt{1+\dfrac{e^2\,e-u|^2}{r^2\,F^2}}}$) continually touching and rol-

ling along the immoveable circle $\overset{\prime\ \prime\prime\ \prime\prime\prime}{PPP}$ &c. (whofe ra-

dius is $\dfrac{rv}{\sqrt{u^2+v^2}}=\dfrac{r}{\sqrt{1+\dfrac{e^2\,u^2}{r^2\,F^2}}}$) fo that the velocity of the

point of contract be $=v=\dfrac{rF}{e}$. Confidering the point Q as always urged from the points $\overset{\prime}{P}$, $\overset{\prime\prime}{P}$, $\overset{\prime\prime\prime}{P}$, &c. and confe-quently its oppofite point (o) towards thofe points, it is neceffary to obferve, that according as u is lefs or greater

than e, the arc $\overset{\prime}{P}$Q (whofe fine is $\dfrac{rv}{\sqrt{e-u|^2+v^2}}$) will be lefs

or greater than 90°; and the point (o) oppofite to Q on the furface of the fphere will accordingly be at a greater or lefs diftance than 90° from $\overset{\prime}{P}$.

If

If u be negative the arc PR whose sine is $\frac{rv}{\sqrt{u^2+v^2}}$ will be greater than 90°.

8. The motion of the sphere according to the regulation in the preceding article is one motion compounded of the primitive motion of the sphere and the motion generated by the action of the force F. But conceiving $\left(\frac{ev}{\sqrt{\overline{e-u}^2+v^2}}\right)$ the velocity of the point Q to arise from an impulse given to it whilst the sphere revolved about an axis of which Q was an immoveable pole before such impulse, and about which the mid-circle corresponding to that primitive axis revolved with the angular velocity $\frac{e.\overline{e-u}}{\sqrt{\overline{e-u}^2+v^2}}$ [a]; and considering that the force F, continually acting at right angles to the momentary direction of the point Q and to the plane of the said mid-circle, only serves to alter the position of the said primitive axis; we may, by the help of what is done above, explain the motion which the sphere will have, during the action of the force F, so as to retain in our ideas the two primitive motions (one about the axis QO, and the other about a diameter at right angles to that axis) as remaining distinct and unaltered.

[a] Denoting this by c and the velocity of Q by d, $\sqrt{c^2+d^2}$ is $= e$, agreeable to art. 1.

Fig.

Fig. 6. Let ED be a great circle on the revolving fphere, of which Q is a pole, and let a fmaller circle DL parallel to (MQ) that which we have found will be defcribed by the point Q, be drawn on the immoveable concave fphere fo as to touch that great circle in the point (D) where the great circle QPR cuts it; the radius of which leffer circle will be $\left(=\sqrt{r^2-\kappa^2}=\right)\dfrac{r v^2 \backsim r u.\overline{e-u}}{\sqrt{u^2+v^2}\times\sqrt{\overline{e-u}^2+v^2}}$.

Then the revolving fphere, during the action of the force F, will fo move, that the firft mentioned great circle (ED) fhall continually touch and roll along the faid leffer circle DL, the velocity of the point of contact (along that circle) being $=\dfrac{v^2 \backsim u.\overline{e-u}}{\sqrt{\overline{e-u}^2+v^2}}$ *(b)*, and the fphere at the fame time turning about the axis of which Q is a pole with the primitive angular velocity $=\dfrac{e.e \backsim u}{\sqrt{\overline{e-u}^2+v^2}}$.

Thus the primitive motion about the axis of which Q is a pole is preferved diftinct, whilft that pole proceeds defcribing a circle, whofe radius is $\dfrac{r e v}{\sqrt{u^2+v^2}\times\sqrt{\overline{e-u}^2+v^2}}$, with the velocity $\dfrac{e v}{\sqrt{\overline{e-u}^2+v^2}}$ which we fuppofed given to it.

(b) This is to the velocity of the point Q as $\sqrt{r^2-\kappa^2}$ to κ; that is, as the radii of the arcs defcribed.

It

It is observable, that the last mentioned velocity will, according to this regulation of the motion, be to the primitive angular velocity about the axis of which Q is a pole, as v to $e-u$, or as v to $u-e$, according as u is less or greater than e; that is, according as the arc $\overset{\prime}{PQ}$ is less or greater than 90°.

9. From what has been said it follows, that denoting the two primitive angular velocities $\dfrac{e.e v u}{\sqrt{\overline{e-u}^2+v^2}}$ and $\dfrac{e v}{\sqrt{\overline{e-u}^2+v^2}}$ (specified in the preceding article) by c and d respectively, the radius (fig. 5.) of the circle $\overset{\prime\;\prime\prime}{ppp}$ &c. (or fine of the arc $\overset{\prime}{PQ}=\overset{\prime\prime}{PQ}$, &c.) will be $=\dfrac{dr}{e}$; the radius of the circle $\overset{\prime\;\prime\prime\;\prime\prime\prime}{PPP}$ &c. (or fine of the arc $\overset{\prime}{PR}=\overset{\prime\prime}{PR}$, &c.) $=$ $\dfrac{dr^2 \text{F}}{e\sqrt{d^2 e^2\mp 2 c dr \text{F}+r^2 \text{F}^2}}$: a great circle passing through the primitive poles o and Q, on the revolving sphere, will turn from the position oRQ with the velocity $\dfrac{r\text{F}}{d}$ measured at the mid-circle, or with the velocity $\dfrac{dr\text{F}}{\sqrt{d^2 e^2\mp 2 c dr \text{F}+r^2 \text{F}^2}}$ measured at the fixed point R; whilst those poles describe, with the velocity d, circles parallel to $\overset{\prime\;\prime\prime}{PPP}$ &c. the radius (κ) of each of the circles (fig. 6.) so described.

2

being

being $= \dfrac{d^2 r}{\sqrt{d^2 e^2 \mp 2 e d r F + r^2 F^2}}$: the radius $\left(\sqrt{r^2 - \kappa^2}\right)$ of the

circle DL will be $= \dfrac{r \times \overline{e d \stackrel{\omega}{\mp} r F}}{\sqrt{d^2 e^2 \mp 2 e d r F + r^2 F^2}}$; and the velocity

$\left(\dfrac{v^2 \times \overline{e \cdot e - n}}{\sqrt{\overline{e - n}^2 + v^2}}\right)$ along the faid circle DL $= c \stackrel{\omega}{\mp} \dfrac{r F}{d}$: the up-

per or lower of the double figns taking place according

as $u \left(= e \mp \dfrac{c r F}{d e}\right)$ is lefs or greater than e; that is, according

as the arc PQ (whofe fine is $= \dfrac{d r}{e}$) is lefs or greater than

90°.

10. As an inftance of the ufe of the preceding con-
clufions, I will now apply them in the folution of a very
interefting problem, which I have not before feen folved.

*Suppofe a given fpheroid, whilft revolving uniformly
about its proper axis, with a given angular velocity, to be
fuddenly urged by fome percuffive force to turn, with fome
given angular velocity, about a diameter of its equator; it
is propofed to explain the rotatory motion of the fpheroid
confequent to the impulfe fo received.*

Fig. 7, 8. Let DOEQ be the fpheroid, whofe femi-axis
CO = CQ is $= b$, and equatorial radius CD = CE $= r$; and
fuppofing it before the impulfe to revolve about its pro-
per axis OQ with the angular velocity c, meafured at the
diftance r from the axis, let the poles (O and Q) be fud-
denly urged by fome percuffive force to turn about a

diameter

diameter of the equator of the fpheroid, with the angular velocity d, likewife meafured at the diftance r from that diameter. Upon receiving fuch impulfe, the fpheroid will take a new axis of motion, which will be a momentary one; fuppofe fuch new axis to be $p c \pi^{(c)}$. Then the particles of the fpheroid being urged (or having a tendency) to turn about that axis with the angular velocity $\sqrt{c^2 + d^2}$, (which we will denote by e) their joint centrifugal force will fo urge the fpheroid to turn about that diameter of the equator which fhall be at right angles to the momentary axis $p c \pi$, that the accelerative force of the point D of the equator to turn it about the faid diameter according to the order of the letters D Q E will (as appears by what is proved in art. 1. and in the Appendix annexed hereto) be $= \frac{c d}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{c d}{r} \times \frac{b^2 - r^2}{r^2 + b^2}$ according as b is lefs or greater than r: and it follows from hence and what is proved in art. 3. and 4. that v, the angular velocity (at the diftance r from c) with which the momentary pole p will change its place, will accordingly be $= \frac{c d}{e} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{c d}{e} \times \frac{b^2 - r^2}{r^2 + b^2}$.

(c) To find the pofition of this axis fee art. 1. by which the fine of the angle $p c p$ (to the radius r) is found $= \frac{d r}{e}$.

Moreover, referring to our obſervation in art. 8. let $u-e$ be to $\frac{cd}{e} \times \frac{r^2-b^2}{r^2+b^2}$ (the value of v) as c to d, u being greater than e; or let $e-u$ be to $\frac{cd}{e} \times \frac{b^2-r^2}{r^2+b^2}$ as c to d, u being leſs than e: whence, in both caſes, we ſhall have the ſame expreſſion $\left(\frac{c^2}{e} \times \frac{r^2-b^2}{r^2+b^2}\right)$ for the value of $u-e$; and conſequently u, in both caſes, will be $= e + \frac{c^2}{e} \times \frac{r^2-b^2}{r^2+b^2}$.

Conceive now a ſpherical ſurface without matter, having the ſame center and radius as the equator DE, to be carried about with the revolving ſpheroid; and ſuppoſe a ſphere, whoſe radius is r, to revolve about an axis p c π with the angular velocity e, and, whilſt it is ſo revolving, let an accelerative force (F) equal to $\frac{cd}{r} \times \frac{r^2-b^2}{r^2+b^2}$ or $\frac{cd}{r} \times \frac{b^2-r^2}{r^2+b^2}$, according as b is leſs or greater than r, urge the pole p, and the ſucceſſive momentary poles as they become ſuch, to turn about a diameter of the contemporary mid-circle in the manner expreſſed in art. 6. u being to v as $e + \frac{c^2}{e} \times \frac{r^2-b^2}{r^2+b^2}$ to $\frac{cd}{e} \times \frac{r^2-b^2}{r^2+b^2}$ or as $e + \frac{c^2}{e} \times \frac{r^2-b^2}{r^2+b^2}$ to $\frac{cd}{e} \times \frac{b^2-r^2}{r^2+b^2}$, according as b is leſs, or greater than r. Then will the motion of the ſurface of this ſphere be exactly the ſame as the motion of the ſaid ſpherical ſurface carried about with the revolving

ſpheroid

fpheroid after receiving the impulfe of the percuffive force. Therefore, having reference to our conclufions in the preceding articles, we, by fubftitution, readily obtain folution to our problem.

By fubftituting properly $\frac{cd}{r} \times \frac{r^2-b^2}{r^2+b^2}$ or $\frac{cd}{r} \times \frac{b^2-r^2}{r^2+b^2}$ for F, we find,

$$\frac{d^2 r}{\sqrt{d^2 e^2 \mp 2cdr\mathrm{F} + r^2 \mathrm{F}^2}} = \frac{dr}{c} \times \frac{r^2+b^2}{\sqrt{\overline{4r^4 + \overline{r^2+b^2}|^2} \times \frac{d^2}{c^2}}},$$

$$\frac{r \times \overline{cd + r\mathrm{F}}}{\sqrt{d^2 e^2 \mp 2cdr\mathrm{F} + r^2 \mathrm{F}^2}} = \frac{2r^3}{\sqrt{\overline{4r^4 + \overline{r^2+b^2}|^2} \times \frac{d^2}{c^2}}},$$

and $c \div \frac{r\mathrm{F}}{d} = \frac{2r^2 c}{r^2+b^2}$.

Which equations, refpect being had to the conclufions in art. 8. and 9. indicate that, whether b be lefs or greater than r, if an immoveable circle DL, whofe radius is $=$

$\frac{2r^3}{\sqrt{4r^4 + \overline{r^2+b}|^2 \times \frac{d^2}{c^2}}}$, be conceived to be defcribed in a plane

inclined to the plane of the equator of the fpheroid (before the impulfe) in an angle whofe fine (to the radius r)

is $= \frac{dr}{c} \times \frac{r^2+b^2}{\sqrt{4r^4 + \overline{r^2+b}|^2 \times \frac{d^2}{c^2}}}$, fo that the faid circle touch

the faid equator in the point D in the fection o*p*D QE; the fpheroid after the impulfe will fo revolve, that its

equator

equator will always touch and roll along the faid immoveable circle (DL), the velocity of the point of contact (along that circle) being $= \frac{2 r^2 c}{r^2 + b^2}$, whilft the fpheroid turns about its proper axis (OQ) with the primitive angular velocity c, and the poles o and Q (by the faid rolling of the equator) defcribe circles (whofe radii are

each $= \frac{b d}{c} \times \dfrac{r^2 + b^2}{\sqrt{4 r^4 + \overline{r^2 + b^2}|^2 \times \frac{d^2}{c^2}}}$) parallel to the faid circle.

DL, with the angular velocity d (or their proper velocity $\frac{b d}{r}$) which we fuppofed given to them by the impulfe[d]. Thus the motion of the fpheroid confequent to the impulfe appears to be remarkably regular.

And in the very fame manner may be explained the motion of a cylinder, whofe primitive motion about its proper axis may be difturbed by fome percuffive force in like manner as we fuppofed the fpheroid difturbed; only (inftead of the former fubftitution for F) fubftituting for the accelerative force arifing from the centrifugal force of the particles of the revolving cylinder its proper value $\frac{c d}{r} \times \frac{3 r^2 - 4 b^2}{3 r^2 + 4 b^2}$ (computed in our Appendix) and afterwards proceeding as we have done with regard to the fpheroid,

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(d) Other ways of folving the problem are alfo fuggefted by the preceding articles.

b denoting half the length of the cylinder, and r the radius of any fection at right angles to its proper axis.

Seeing that $\left(\frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}\right)$ the expreffion for the faid accelerative force refpecting the cylinder vanifhes when b is $= \frac{3^{\frac{1}{2}} r}{2}$, it is manifeft that the cylinder in that cafe will (with refpect to its own particles) undifturbedly revolve about any axis whatever paffing through its center of gravity, as will a fphere. Which remarkable property of that particular cylinder I believe has not before been taken notice of.

There are, I am aware, bodies of other forms having the like property.

The preceding articles lead us to confider the motion of the earth's axis in a light, I prefume, more clear and fatisfactory than any in which it has before been confidered; but I muft, for want of leifure, defer making the application till fome future opportunity; only obferving here, that by what is done above it appears, that from the action of the Sun and Moon on the earth its axis has a diurnal motion, which I have no where feen explained. Which motion is not much unlike that of the axis of the revolving fpheroid juft now confidered, when $(2b)$ this laft mentioned axis is many times longer than $(2r)$

the

Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
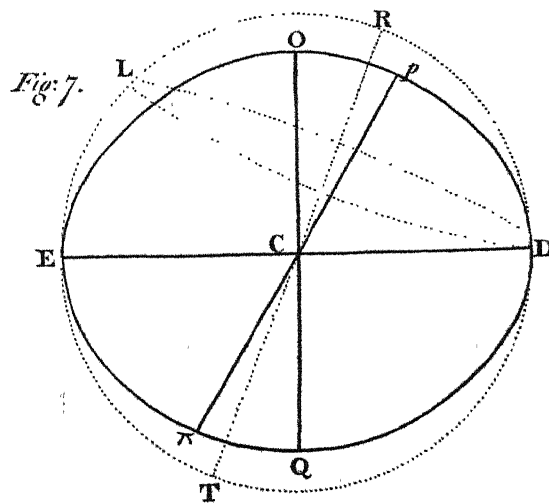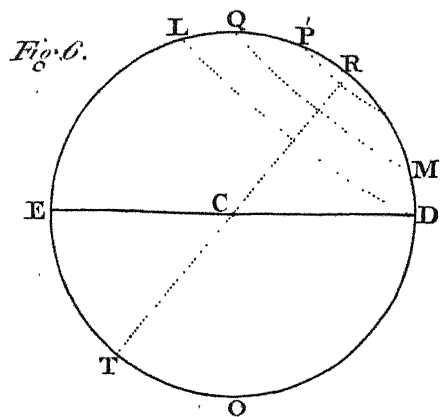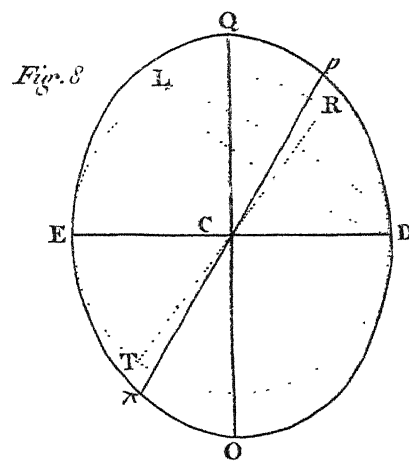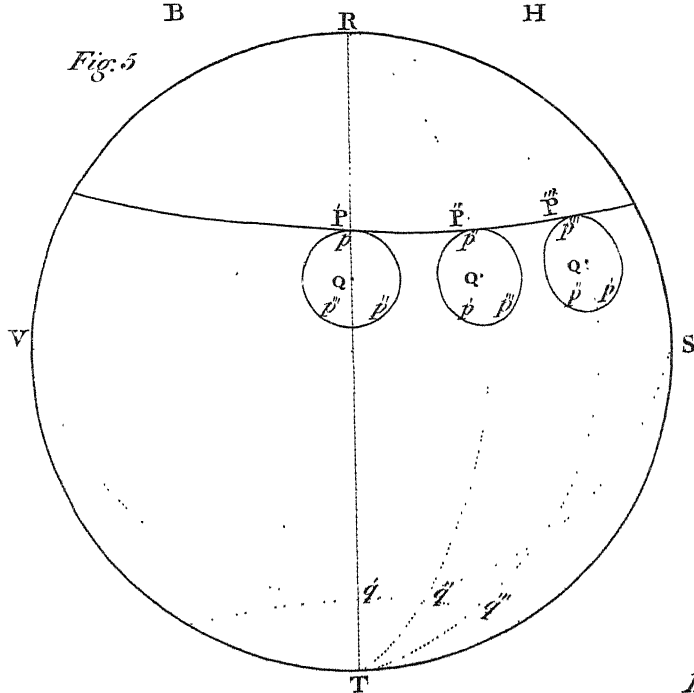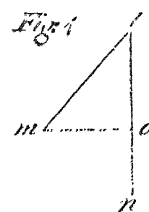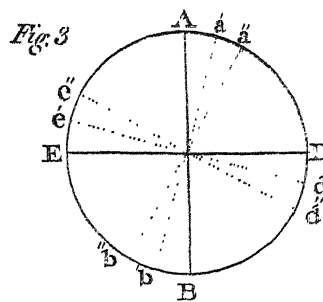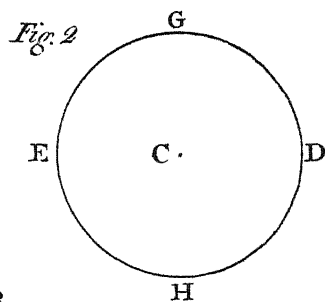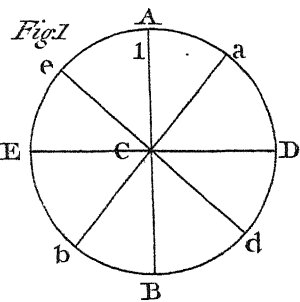
the equatorial diameter of the faid fpheroid, and $\frac{d}{c}$ very fmall.

A P P E N D I X.

Shewing how the joint centrifugal force of the particles of a fpheroid or cylinder, having a rotatory motion about any momentary axis, is computed.

1. FIG. 9. Let p be a particle of matter firmly connected with the plane DOEFQG, in which the line OCQ is fituated; and pq being a perpendicular from p to the faid plane, let the diftance pq be denoted by u; alfo, the line ql being at right angles to OlCQ, let the diftance pl be denoted by h. Then, the faid plane with the particle p being made to revolve about OlCQ as an axis, with the angular velocity e meafured at the diftance a from the faid axis, the velocity of p will be $= \frac{he}{a}$, and its centrifugal force from l will (by a well-known theorem) be $= \frac{he^2}{a^2}$ to make it a^2 the expreffion being $\frac{he^2}{a^2} \times p$. Whence, by refolving that force into two others, one in the direction qp, and the other in a direction parallel to lq, it appears that the force urging p from the plane DOEFQG will be $= \frac{ue^2}{a^2} \times p$, let the diftance lq be what it will.

2. The

2. The particle p being connected with the plane DOEFQG as mentioned in the preceding article, and the diſtance cl being denoted by v; if p be urged directly from the ſaid plane by a force $fu \times p$, the efficacy of that force to turn the ſaid plane about the line HCI, therein drawn at right angles to OCQ, will (by the property of the lever) be equivalent to the force $\frac{fuv \times p}{g}$ acting on the ſaid line OCQ at right angles to the ſaid plane at the diſtance g from the point c.

Moreover it is obvious, that, *cæteris paribus*, the efficacy will be the ſame let the diſtance of q from l be what it will.

Fig. 10. Let q coincide with l; and let ck be a line in the plane clp continued (which plane will be at right angles to the plane DOEFQG); alſo, pk being at right angles to ck, let thoſe lines pk and ck be denoted by w and x reſpectively. Then the ſine and coſine of the angle $k$co to the radius 1, being reſpectively denoted by m and n, the force $\frac{fuv \times p}{g}$ will be $= \frac{f \times p}{g} \times \overline{mn \times \overline{w^2 - x^2} + \overline{m^2 - n^2} \times wx}$.

Conſequently, if each particle of any ſolid body, through which a line HCI and a plain DOEIFQGH may be conceived to paſs, be urged from that plane by a force expreſſed by $fu \times p$ as above; the force which, acting on the line OCQ at the diſtance g from c, would be equivalent to the efficacy

efficacy of all the forces acting on the several particles of that body to turn the same about the line HCI will be obtained by computing the sum of all the forces $\frac{f \times p}{g} \times \overline{m\,n \times \overline{w^2 - x^2} + \overline{m^2 - n^2} \times w\,x}$ acting on the said body.

The computation of such equivalent force will in most cases be abridged by observing that, if $p\,k$ be continued to $\overset{''}{p}$ so that $k\overset{''}{p}$ be $= k\,p$, the efficacy of the force on the particle $\overset{''}{p}$, to turn the body about the line HCI in opposition to the force on the particle p, will be represented by the equivalent force $\frac{f \times \overset{''}{p}}{g} \times \overline{m\,n \times \overline{x^2 - w^2} + \overline{m^2 - n^2} \times w\,x}$ acting on the line OCQ at the distance g from C; and that therefore the efficacy of the two forces on p and $\overset{''}{p}$, to turn the body about HCI, will be represented by the equivalent force $\frac{2f \times p}{g} \times \overline{m\,n \times \overline{w^2 - x^2}}$ acting on the line OCQ, at right angles to the plane DOEIFQGH, at the distance g from C.

3. Fig. 11, 12. If the body be a cylinder, a spheroid, or the like, and its proper axis be situated in the line $c\,k$, the ordinates corresponding the *abscissæ* $k\,p$, $k\overset{''}{p}$, in the circular section $h\,i$ whose center is k, will each be parallel to that diameter passing through c, about which the body will be urged to turn; and each of those ordinates will be $= \sqrt{y^2 - w^2}$, y being the radius of such section.

Therefore,

Therefore, writing $2\sqrt{y^2-w^2}$ inftead of p, it follows that

$\dfrac{4Af}{g}\times mn\dot{x}\times\overline{\dfrac{y^4}{4}-x^2y^2}$, the whole fluent of $\dfrac{4f\times\sqrt{y^2-w^2}}{g}\times$

$mn\dot{x}\times\overline{w^2-x^2}\times\dot{w}$, generated $w \ (=kp=k\ddot{p})$ from o becomes equal to the radius y (both x and y being confidered as invariable) will exprefs the value of the force which, acting on the line ocq at the diftance g from c, would be equivalent to the force of all the particles in the faid fection, whofe thicknefs is denoted by the indefinitely fmall quantity $\dot{x}$; the diftance ck being denoted by x, and A being put for $(.78539)$ the area of a quadrant of a circle whofe radius is 1.

4. Fig. 11. In the cylinder whofe length is $2b$ and diameter $2r$; y being $= r$, $\dfrac{y^4}{4}-x^2y^2$ will be $= r^2\times\overline{\dfrac{r^2}{4}-x^2}$: confequently, the fluent of $\overline{\dfrac{r^2}{4}-x^2}\times\dot{x}$, generated whilft x from o becomes $= b$, being $\dfrac{br^2}{4}-\dfrac{b^3}{3}$, we have $\dfrac{8Afr^2}{g}\times mn\times$

$\overline{\dfrac{br^2}{4}-\dfrac{b^3}{3}}=\dfrac{fmn}{12g}\times\overline{3r^2-4b^2}\times$ M for the force which, acting as above at the diftance g from (c) the center of gravity of the cylinder, would be equivalent to the efficacy of the forces acting as above on all the particles of the cylinder to turn it about a diameter paffing through c, M being the mafs or content of the cylinder.

7

5. Fig.

5. Fig. 12. In the ſpheroid whoſe proper axis is $2b$ and equatorial diameter $2r$, y^2 being $= \frac{r^2}{b^2} \times \overline{b^2 - x^2}$, $\frac{y^4}{4} - x^2 y^2$ will be $= r^2 \times \overline{\frac{r^2}{4} - \frac{r^2 x^2}{2b^2} + \frac{r^2 x^4}{4b^4} - x^2 + \frac{x^4}{b^2}}$: conſequently, the fluent of $\frac{r^2 \dot{x}}{4} - \frac{r^2 x^2 \dot{x}}{2b^2} + \frac{r^2 x^4 \dot{x}}{4b^4} - x^2 \dot{x} + \frac{x^4 \dot{x}}{b^2}$, generated whilſt x from o becomes $= b$, being $\frac{r^2 b}{4} - \frac{r^2 b}{6} + \frac{r^2 b}{20} - \frac{b^3}{3} + \frac{b^3}{5} = \frac{2}{15} \times \overline{r^2 b - b^3}$, we have $\frac{16 A f r^2}{15 g} \times mn \times \overline{r^2 b - b^3} = \frac{f m n}{5 g} \times \overline{r^2 - b^2} \times$ s for the force which, acting at the diſtance g from c the center of the ſpheroid, would be equivalent to the efficacy of the forces acting as above on all the particles of the ſpheroid to turn it about a diameter of its equator, s being the maſs or content of the ſpheroid.

Theſe equivalent forces are diſtinguiſhed by the name of motive forces; the correſpondent accelerative forces are computed in the following articles.

6. Fig. 13. The body being a ſpheroid whoſe center is c, and whoſe proper axis PN is $= 2b$ and equatorial diameter AB $= 2r$; let F be the accelerative force of a particle at the diſtance g from the axis about which the body is urged to turn, which axis is ſuppoſed to be a diameter of its equator. Denote ck by x; ki by y; and let the *abſciſſa* ko and its correſpondent ordinate (parallel to the laſt mentioned axis) in the circle whoſe radius is ki be denoted

denoted by s and t respectively. Then, considering the body as urged to turn about that diameter of its equator which is at right angles to AB, the accelerative force of every particle in the said ordinate will be $= \frac{\sqrt{s^2+x^2}}{g} \times$ F, and the motive force of all the particles in the same ordinate will be $= \frac{\sqrt{s^2+x^2}}{g} \times \mathrm{F}.\dot{t}\dot{s} = \frac{\sqrt{s^2+x^2}}{g} \times \mathrm{F}\dot{s} \sqrt{y^2-s^2}$; to which (by the property of the lever) a motive force $= \frac{s^2+x^2}{g^2} \times \mathrm{F}\dot{s}\sqrt{y^2-s^2}$ acting at the distance g from the center at right angles to a ray therefrom would be equivalent. Therefore, considering x and y as invariable, and s only as variable, $\frac{4\mathrm{F}\dot{x}}{g^2} \times$ the whole fluent of $\dot{s}\sqrt{y^2-s^2} \times \overline{s^2+x^2}$ will denote a force which, acting at the distance g from c, would be equivalent to the motive force of all the particles in the section bi whose radius is ki and thickness $\dot{x}$. Which fluent is $= \mathrm{A}y^2 \times \overline{x^2+\frac{y^2}{4}} = \frac{\mathrm{A}r^2}{b^2} \times \overline{b^2-x^2} \times \overline{x^2+\frac{r}{4b}} \times \overline{b^2-x^2}$. Consequently $\frac{8\mathrm{A}r^2\mathrm{F}}{b^2 g^2} \times$ the whole fluent of $\dot{x} \times \overline{b^2-x^2} \times \overline{x^2+\frac{r^2}{4b^2}} \times \overline{b^2-x^2}$ will denote a motive force which, acting at the distance g from c at right angles to a ray therefrom, would be equivalent to the whole motive force urging the spheroid to turn as above mentioned. Such equivalent

                     force

Fig.9

Fig.10

Fig.11

Fig.12

Fig.13

Fig.14
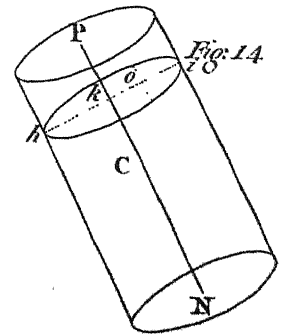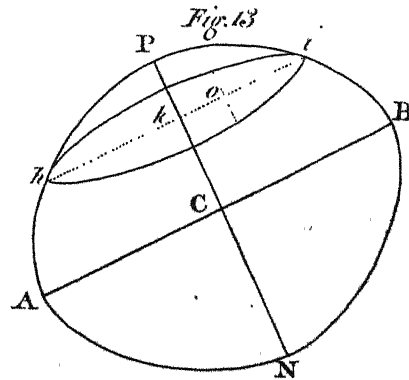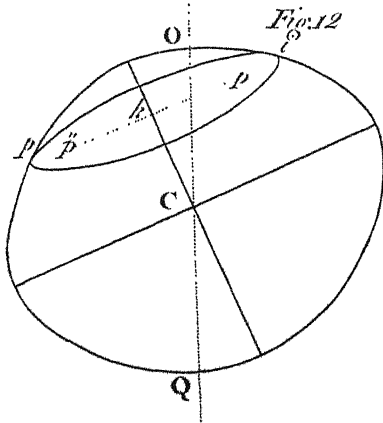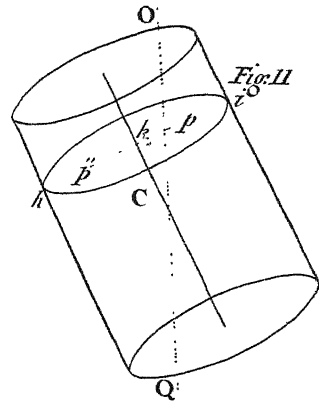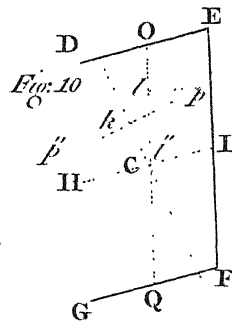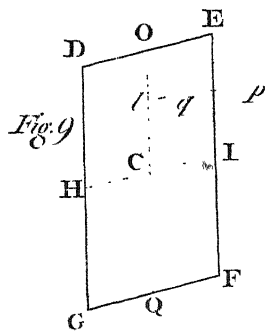
force will therefore be $= \frac{16\,A\,r^2\,F}{15g^2} \times \overline{r^4 b + r^2 b^3} = \frac{F}{5g^2} \times \overline{r^2 + b^2} \times s$;

and this being put $= \frac{fmn}{5g} \times \overline{r^2 - b^2} \times s$ (the value of the same force found in art. 5.) we find $F = fg\,mn \times \frac{r^2 - b^2}{r^2 + b^2}$;

which will be $= \frac{gmne^2}{a^2} \times \frac{r^2 - b^2}{r^2 + b^2}$, if f be $= \frac{e^2}{a^2}$, its value computed in art. 1.

Or F will be denoted by $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$; if 1 be to e as m to d, and as n to c; and a and g be each $= r$.

7. Fig. 14. The body being a cylinder whose center of gravity is in c, and whose proper axis PN is $2b$ and diameter $2r$; the accelerative force (F) at the distance g from c, will in like manner be found $= \frac{gmne^2}{a^2} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$; the cylinder being considered as urged to turn about a diameter passing through c.

If $1 : e :: m : d :: n : c$, and a and g be each $= r$, F will be $= \frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$;

XVI. *Directions for making the best Composition for the Metals of reflecting Telescopes; together with a Description of the Process for grinding, polishing, and giving the great Speculum the true parabolic Curve. By Mr.* John Mudge; *communicated by* Alexander Aubert, *Esq. F. R. S.*

Read Feb. 27. March 6. and 13. 1777.

AS the method of casting, grinding, and polishing the specula of reflecting telescopes, by Mess. MOLYNEUX and HADLEY, which is published in Dr. SMITH's Optics, is what the workmen have generally followed, and is consequently well known to them; I shall in the following account avoid a repetition of the general directions there given, and only remark upon such parts of that process which I think are essentially defective, and supply them by a method of my own, which, from long and repeated trials, I have found completely to answer the purpose. After, therefore, referring to the above account for the manner of making the gages, patterns, the method of casting, as well as a great many other particulars, I will begin with

The

The beſt compoſition for the ſpecula of reflecting teleſcopes.

The perfection of the metal of which the ſpeculum ſhould be made conſiſts in its hardneſs, whiteneſs, and compactneſs; for upon theſe properties the reflective powers and durability of the ſpeculum depend. And firſt of the hardneſs and whiteneſs of the metal. There are various compoſitions recommended in SMITH's Optics, all which have however their ſeveral defects. Three parts copper and one part and one-fourth of tin will make, he ſays, a very hard white metal; but it is liable to be porous. This, however, is an imperfection which I ſhall preſently ſhew the method of preventing; but the permanent fault of it, and which I have myſelf experienced, is, that it is not hard enough. The ſpeculum of a reflecting teleſcope ought to have the utmoſt poſſible hardneſs, compatible with its being operated upon by the tool.

It is to be obſerved, that ever ſo ſmall a quantity of tin added to melted copper deſtroys its perfect malleability, and at the ſame time produces a metal whiter and harder than copper. As the quantity of tin is increaſed, ſuppoſe to a fifth or fourth part, the metal becomes whiter, ſtill harder, and conſequently more friable. If the quantity of tin be further increaſed to a third of the

whole compoſition, it will then have its utmoſt whiteneſs; but will be rendered at the ſame time ſo exceedingly hard and brittle, that the fineſt waſhed emery upon lead or braſs will not cut it without breaking up its ſurface; and the common blue ſtones uſed in grinding the ſpeculum, will not touch it. Mr. JACKSON (ſome time ſince dead) a mathematical-inſtrument-maker, and a moſt excellent workman, told me, that the tin was increaſed to the above proportion in his metals; but that they were ſo exceedingly hard, that it coſt him an infinite deal of pains, and a journey of two hundred miles, to find out a ſtone of ſufficient hardneſs to cut it, and whoſe texture at the ſame time was fine enough not to injure its ſurface. I have ſeen ſeveral of his finiſhed metals; they were indeed perfectly hard and white; but the kind of ſtone with which he ground them he kept a ſecret.

After many experiments with various proportions of tin and copper, by gradually increaſing the former, I at laſt found that fourteen ounces and an half of grain-tin to two pounds of good Swediſh copper, made a beautiful white and very hard metal; ſo hard indeed, that the ſtones would but barely cut it, and waſhed emery on braſs or tin but juſt grind the ſurface without breaking it up; whereas the proportion of tin being increaſed by the addition of only another half ounce, the former inconvenience

venience immediately took place. This therefore is the *maximum* in point of hardnefs.

Thus much of the two firſt confiderations, the hard-nefs and whitenefs of the metal; the next, and indeed the moſt eſſential, property is its compactnefs, or its being without pores.

This compoſition (though complete in the former refpects) was, as well as Dr. SMITH's, fubject every now and then to be porous; fometimes, indeed, I fucceeded in caſting a ſingle metal, or perhaps two or three, without this imperfection; at other times, and moſt frequently indeed, they were attended with this defect, without my being at all able to form a probable conjecture at the caufe of my fuccefs or difappointment. The pores were fo very fmall that they were not difcoverable when the metal had received a good face and figure upon the hones, nor till the laſt and higheſt poliſh had been given; and then it frequently appeared as if duſted over with millions of microfcopic pores, which were exceedingly prejudicial in two refpects; for firſt, they became in time a lodg-ment for a moiſture which tarniſhed the furface; and fecondly, on poliſhing the fpeculum, the putty neceſſa-rily rounded off the edges of the pores, fo as to fpoil a great part of the metal, by the lofs of as much light and

ſharpnefs

ſharpneſs in the image as there were defective points of reflection in the metal.

Beſides the trouble of a great number of experiments, in order to get rid of this miſchief, and to aſcertain the cauſe to which it was owing, there was this additional inconvenience attending it, *viz.* that the fault was not diſcovered, as was obſerved before, till a great deal of trouble had been taken in grinding and even poliſhing the metal, the whole of which was rendered uſeleſs by the mortifying diſcovery of this defect.

I was extricated at laſt from this difficulty, and in ſome meaſure by accident. Having one day made a great number of experiments, and having melted down all the good copper I had or could procure; though puzzled and fatigued, yet not caring to give it up, I recollected that I had ſome metal which was reſerved out of curioſity, and was a part of one the bells of St. Andrew's which had been re-caſt. Expecting, however, very little from this groſs and uncertain compoſition, I was nevertheleſs determined to ſee what could be made of it by enriching the compoſition with a little freſh tin. Accordingly caſting a metal with it, it turned out perfectly free from pores, and in every reſpect as fine a metal as ever I ſaw.

I could not at firſt conceive to what this ſucceſs was owing; but at laſt I hit upon the real cauſe of that defect,

                which

which had given me fo much embarraffment and trouble during a courfe of near a hundred experiments, and in confequence thereof fell upon a method which ever after prevented it.

I had hitherto always melted the copper firft, and when it was fufficiently fufed, I ufed to add the proportional quantity of tin; and as foon as the two were mixed, and the fcoria taken off, the metal was poured into the moulds. I began to confider that putty was calcined tin, and ftrongly fufpected, that the exceffive heat which the copper neceffarily undergoes before fufion, was fufficient to reduce part of the tin to this ftate of calcination, which therefore might fly off from the compofition in the form of putty, at the time the metal was poured into the flafks.

Upon this idea, after I had furnifhed myfelf with fome more Swedifh copper and grain-tin (both which I had always before ufed) I melted the copper, and having added the tin as ufual to it, caft the whole into an ingot: this was, as I expected, porous. I then melted it again, and as in this mixed ftate it did not acquire half the heat which was before neceffary to melt the copper alone, fo it was not fufficient to calcine the tin; the fpeculum was then perfectly clofe, and free from this fault; nor did I ever after, in a fingle inftance, meet with the above mentioned imperfection.

All

All that is neceffary, therefore, to be done to procure a metal which fhall be white, as hard as it can be wrought, and perfectly compact, is to melt two pounds of Swedifh copper, and when fo melted, to add fourteen ounces and a half of grain-tin to it; then, having taken off the fcoria, to caft it into an ingot. This metal muft be a fecond time melted to caft the fpeculum; but as it will fufe in this compound ftate with a fmall heat, and therefore will not calcine the tin into putty, it fhould be poured off as foon as it is melted, giving it no more heat than is abfolutely neceffary. It is to be obferved, however, that the fame metal, by frequent melting, lofes fomething of its hardnefs and whitenefs: when this is the cafe, it becomes neceffary to enrich the metal by the addition of a little tin, perhaps in the proportion of half an ounce to a pound. And indeed when the metal is firft made, if inftead of adding the fourteen ounces and a half of tin to the two pounds of melted copper, about one ounce of the tin were to be referved and added to it in the fucceeding melting, before it is caft off into the moulds, the compofition would be the more beautiful, and the grain of it much finer: this I know by experience to be the cafe.

The beft method for giving the melted metal a good furface is this: the moment before it is poured off, throw into the crucible a fpoonful of charcoal-duft; imme-

diately

diately after which the metal muſt be ſtirred with a wooden ſpatula, and poured into the moulds.

I wiſh I may not be conſidered as tedious in the above detail; but as this buſineſs cauſed me a great deal of trouble, I was willing to give ſome account of the means by which I was freed from this difficulty ever after. Perhaps, indeed, the whole of this proceſs may be unne-ceſſary, as many years ſince, I communicated this com-poſition, and I believe at the ſame time the method of preventing the pores, to the late Mr. PETER COLLISON, a member of the Royal Society; and likewiſe two or three years ſince, at the deſire of my brother, to Mr. MICHELL. Although it be poſſible, therefore, that this method is generally known, yet, as I have frequently of late ſeen ſpecula with this defect, and obſerved metals of ſome of Mr. SHORT's teleſcopes which are not quite ſo per-fect as could be wiſhed (though they are all exqui-ſitely figured) I was willing by this publication wholly to remove any future embarraſſment of this ſort, and to furniſh workmen with an excellent compoſition for their metals. And would the Royal Society be pleaſed to honour the proceſs with a place in their records, I know of no other method ſo proper to give this, as well as the following information, a general notoriety.

The metal being caſt, there will be no occaſion for the complicated apparatus directed by Dr. SMITH, for

<div align="right">grinding</div>

grinding and poliſhing it. Four tools are all that are
neceſſary, *viz.* the rough grinder to work off the rough
face of the metal; a braſs convex grinder, on which the
metal is to receive its ſpherical figure; a bed of hones
which is to perfect that figure, and to give the metal a
fine ſmooth face; and a concave tool or bruiſer, with
which both the braſs grinder, and the hones are to be
formed. A poliſher may be conſidered as an addi-
tional tool; but as the braſs grinder is uſed for this pur-
poſe, and its pitchy ſurface is expeditiouſly, and without
difficulty formed by the bruiſer, the apparatus is there-
fore not enlarged.

Of rough grinding the ſpeculum.

The tool by which the rough ſurface of the metal is
rendered ſmooth and fit for the hones, is beſt made of lead,
ſtiffened with about a fifth or ſixth part of tin. This tool
ſhould be at leaſt a third more in diameter than the metal
which is to be ground; and for one of any ſize, not leſs
than an inch thick. It may be cemented upon a block
of wood, in order to raiſe it higher from the bench.

This leaden tool being caſt, it muſt be fixed in the lathe,
and turned as true as it is poſſible, by the gage, to the
figure of the intended ſpeculum, making a hole or pit in
the

the middle, as a lodgment for the emery, of about an inch diameter for a metal of four inches: when this is done, deep grooves muſt be cut acroſs its ſurface with a graver, in the manner repreſented in fig. 1. Theſe grooves will ſerve to lodge the emery, and by their means the tool will cut a great deal faſter. There is no occaſion to fear any alteration in the convexity of this tool by working the metal upon it, for the emery will bed itſelf in the lead, and ſo far arm the ſurface of it, that it will preſerve its figure and cut the metal very faſt. Any kind of low handle, fixed on the back of the metal with ſoft cement, will be ſufficient; but it ſhould cover two-thirds of its back to prevent its bending. This way of working will cut the metal faſter, and with more truth, than the method deſcribed by Dr. SMITH; for ſhould the ſurface and rough parts be attempted to be ground off by a common grind-ſtone by hand, though you did it as near the gage as poſſible, yet the metal would be ſo much out of truth when applied to the ſuccceeding tool, that no time would be ſaved by it. I uſed to employ a common labourer for this purpoſe, who ſoon acquired ſuch a dexterity at working upon this tool, that in two hours time he would give a metal of four inches diameter ſo good a face and figure as even to fit it for the hones. When all the ſand-holes and irregularities on the face of the metal are ground off, and the whole ſurface

is ſmooth and regularly figured, the ſpeculum is then ready for the braſs grinder, and muſt be laid aſide for the preſent.

The manner of forming the braſs-grinding tool.

The following is the method I have always purſued. Procure a round ſtout piece of Hamburgh braſs, at moſt a ſixth part larger than the metal to be poliſhed; and let it be well hammered into a degree of convexity (by the aſſiſtance of the gage) ſuitable to the intended ſpeculum. Having done this, ſcrape and clean the concave ſide ſo thoroughly that it may be well tinned all over; then caſt upon it, after it has been preſſed a proper depth into the ſand, the former compoſition of tin and lead, in ſuch quantity, that it may (for a ſpeculum of four inches diameter) be at leaſt an inch and an half thick, and with a baſe conſiderably broader than the top, in order that it may ſtand firmly upon the bench in the manner hereafter to be deſcribed. This being done, it muſt be fixed and turned in the lathe with great care, and of ſuch a convexity as exactly to ſuit the concave gage, which we ſuppoſe already made. It will be neceſſary to be more careful in forming this than the former tool, and eſpecially that no rings be left from the turning;

I   **nor**

nor will the fucceeding hone tool require fo much exact-
nefs, as any defects in turning, will, by a method hereafter
mentioned, be eafily remedied; but any inequality or want
of truth in the brafs tool will occafion a great deal of trou-
ble before it can be ground out by the emery. This tool
muft have a hole (fomewhat lefs than that in the metal
to be worked upon it) in the middle, quite through to
the bottom. When this tool is finifhed off in the lathe,
its diameter fhould be one-eighth wider than the metal.

How to form the bed of hones, or the third tool.

Having chofen the kind of hones, and the beft too, of
the fort recommended in SMITH's Optics; they fhould
be cemented in fmall pieces (in a kind of pavement
agreeably to his directions) upon a thick round piece of
marble, or metal made of lead and tin like the former
compofition (which is what I have always ufed) in fuch
a manner, that the lines between the ftones may run
ftraight from one fide to the other; fo that, placing the
teeth of a fine faw in each of thefe divifions, they may
be cleared from one end to the other of the cement
which rifes between the ftones. This bed of hones
fhould be at leaft a fourth part larger than the metal
which is to be ground upon it. The furface of the

metal

metal upon which the hone pavement is to be cemented may or may not, as you pleaſe, be turned of a convexity ſuitable to the gage, though I have never taken that trouble. As ſoon as the hones are cemented down, and the joints cleared by the ſaw, this tool muſt be fixed in the lathe, and turned as exactly true to the gage as poſ-ſible; which done, it muſt be laid aſide for the preſent. The next tool to be made is the bruiſer.

The manner of forming the bruiſer, the fourth and laſt tool.

This tool ſhould be likewiſe made of thick ſtout braſs like the former, perfectly found, about a quarter of an inch thick, and hammered as near to the gage as poſſible. It ſhould be then ſcraped, cleaned, and tinned on the convex ſide, as the former tool was on the concave, and the ſame thickneſs of lead and tin caſt upon it. The general ſhape of this ſhould differ from the former; for as that increaſed in diameter at the bottom for the ſake of ſtanding firmly, ſo this ſhould be only as broad at bottom as at top, as it is to be uſed occaſionally in both thoſe poſitions. When this tool is fixed in the lathe, and turned off concave to the convex gage with great truth likewiſe,

likewife, its diameter ought to be the middle fize between the hones and the polifher.

Having with the lathe roughly formed the convex brafs grinder, the bed of hones, and the concave bruifers, the convex and concave brafs tools and the metal muft be wrought alternately and reciprocally upon each other with fine emery and water, fo as to keep them as nearly to the fame figure as poffible, in order to which fome wafhed emery muft be procured. This is beft done by putting it into a phial, which muft be half filled with water and well fhaken up, fo that, as it fubfides, the coarfeft may fall to the bottom firft, and the fineft remain at the top: and whenever frefh emery is laid on the tools, the beft method (which we fhould alfo obferve with the putty in polifhing) will be, to fhake gently the bottle, and pour out a fmall quantity of the turbid mixture.

Of grinding the fpeculum, the brafs tool, and the bruifer, together.

All the tools being ready, upon a firm poft in the middle of a room, you are to begin to grind the brafs convex tool with the bruifer upon it, working the latter crofsways, with ftrokes fometimes acrofs its diameter, at others a little to the right and left, and always fo fhort that the

bruifers

bruifers may not pafs above half an inch within the fur-face of the brafs tool either way, fhifting the bruifer round its axis every half dozen ftrokes or thereabout. You muft likewife, every now and then, fhift your own pofition, by walking round, and working at different fides of the brafs tool; at times the ftrokes fhould be carried round and round, but not much over the tool: in fhort, they muft be directed in fuch a way, and the whole grinding conducted in fuch a manner and with fuch equability, that every part of both tools may wear equally. This habit of grinding, as well as the future one of polifhing, will be foon acquired. When you have wrought in this manner about a quarter of an hour with the bruifer upon the tool, it will be then neceffary to change them, and, placing the bruifer upon its bottom, to work the convex tool upon that in the fame manner.

When by working in this equable manner, alternately with the bruifer and tool, and occafionally adding frefh emery, you have nearly got out all the veftiges of the turning tool, and brought them both nearly to a figure, it will be then time to give the fame form to the metal. This muft be done by now and then grinding it upon the brafs tool with the fame kind of emery, taking care however, by working the two former tools frequently together, to keep all three exactly in the fame curve.

The

The beft kind of handle for the metal is made of lead, a little more than double its thicknefs, and fomewhat lefs in diameter, of about three pounds weight, with a hole in the middle (for reafons to be fhewn hereafter) a little larger than that in the metal: this handle fhould be cemented on with pitch. The upper edge of this weight muft be rounded off, that the fingers may not be hurt; and a groove, about the bignefs of the little finger, be turned round juft below it, for the more conveniently holding and taking the metal off the tools.

The manner of figuring the metal upon the hones.

When the bruifer, brafs tool, and metal, are all brought to the fame figure, and have all a true good furface, the next part of the procefs is to give a correct fpherical figure and a fine face to the metal, upon the hones. It will be neceffary to premife, however, that the hones fhould be placed in a veffel of water, with which they fhould be quite covered for at leaft an hour before they are ufed, otherwife they will be perpetually altering their figure when the metal comes to be ground upon them. The fame precaution is alfo neceffary, if you are called off from the work while you are grinding.

grinding the metal, for if they be fuffered to grow dry, the fame inconvenience will arife.

In order to give a proper figure to the hones, and exactly fuitable to that of the brafs tool, bruifer, and metal, when the hones are fixed down to the block, fome common flour emery (unwafhed) with a good deal of water muft be put upon them, and the bruifer being placed upon the hones and rubbed thereon with a few ftrokes and a light hand, the inequalities of the ftone will be quickly worn off; but as a great deal of mud will be fuddenly generated, it muft be wafhed off every quarter of a minute with a great deal of water. By a repetition of this, two or three times, the hones (being of a very foft and friable fubftance) will be cut down to the figure, without wearing or altering the bruifer at all. Though this bufinefs may be quickly done, and can be continued but for a few ftrokes at a time, I need not fay that it is neceffary that thofe ftrokes be carried in the fame direction, and with the fame care, which was obferved in grinding the former tools together.

As foon as the hones have received the general figure of the bruifer, and all the turning ftrokes are worn out from them, the emery muft be carefully wafhed off; in order to which, it will be neceffary to clear it from the joints with a brufh under a ftream of water. The bruifer

2

and metal muſt be likewiſe cleared in the ſame manner, and with equal care, from any lurking particles of emery.

The hones being fixed down to the block, you now begin to work the bruiſer upon them with very cautious, regular, ſhort ſtrokes, forward and backward, to the right and left, turning the axis of the bruiſer in the hand while you move round the hones, by ſhifting your poſition, and walking round the block. Indeed the whole now depends upon a knack in working, which ſhould be conducted nearly in the following manner. Having placed the bruiſer on the centre of the hones, ſlide it in an equable manner forward and backward, with a ſtroke or two directly acroſs the diameter, a little on one ſide, and ſo on the other; then ſhifting your poſition an eighth part round the block, and having turned the bruiſer in your hand about as much, give it a ſtroke or two round and round, but not far over the edges of the hones, and then repeat the croſs ſtrokes as before: thoſe round ſtrokes (which ought not to be above two or three at moſt) are given every time you ſhift your own poſition and that of the metals, previous to the croſs ones, in order to take out any ſtripes either in the hones or bruiſer, which may be ſuppoſed to be occaſioned, by the ſtraight croſs ſtrokes. During the time of working, no mud muſt be ſuffered to collect upon the hones, ſo as to

deftroy the perfect contact between the two tools; and therefore they muft every now and then be wafhed clean by throwing fome water upon them. When by work-ing in this manner all the emery ftrokes are ground off from the bruifer, and it has acquired a good figure and clean furface, you may then begin with the metal upon the hones, in the fame cautious manner, wafhing off the mud as faft as it collects, though that will be much lefs now than when the bruifer was ground upon them. Every now and then, however, the bruifer muft be rub-bed gently and lightly upon the hones, which will as it were, by fharpening them and preventing too great fmoothnefs, occafion them to cut the metal much fafter.

When, after having fome time cautioufly wrought in the manner before defcribed, the hone-pavement has uniformly taken out all the emery ftrokes, and given a fine face and true figure to the metal, which will be pretty well known by the great equality there is in the feel while you are working, and by which an expe-rienced workman will form a pretty certain judgment; having proceeded thus far, I fay, you may then try your metal, and judge of its figure by this more certain manner.

Wafh the hone pavement quite clean; then put the metal upon the center of it, and give two or three light

<div align="right">ftrokes</div>

ftrokes round and round only, not carrying, however, the edges of the metal much over the hones; this will take out the order of ftraight ftrokes: then having again wafhed the hones, and placed the fpeculum upon their center, with gentle preffure, flide it towards you till its edge be brought a little over that of the hones, then carry it quite acrofs the diameter as far the other fide, and having given the metal a light ftroke or two in this direction, take it off the tool. The metal being wiped quite dry, place it upon a table at a little diftance from a window; ftand yourfelf as near the window, at fome diftance from the metal, and looking obliquely on its furface, turn it round its axis, and you will fee at every half turn the grain given by the laft crofs ftrokes flafh upon your eye at once over the whole face of the metal. This is as certain a proof of a true fpherical figure as the operofe and difficult method defcribed in Dr. SMITH's Optics; for as there is nothing foft or elaftic, either in the metal or in the hones, this glare is a certain proof of a perfect contact in every part of the two furfaces; which there could not be if the fpheres were not both perfect and precifely the fame.

Indeed there is one accidental circumftance which neceffarily affords its aid in this and every bufinefs of the like fort; and that is, that a concave and convex furface

ground

ground together, though ever fo irregular at firft, will
(if the working be uniform and proper, confifting, efpe-
cially at laft, of crofs ftrokes in every poffible direction
acrofs the diameter) be formed into portions of true and
equal fpheres; had it not been for this lucky neceffity, it
would have been impoffible to have produced that cor-
rectnefs which is effential in the fpeculum of a good re-
flecting telefcope by any mechanic contrivance what-
ever. For when it is confidered, that the errors in reflec-
tion are four times as great as in refraction, and that the
leaft defect in figure is magnified by the powers of the
inftrument, any thing fhort of perfection in the figure
of the fpeculum would be evidently perceived by a want
of diftinctnefs in the performance.

I muft not, however, quit this article without obferv-
ing, that I all along fuppofe, both in forming the tools,
and at laft figuring the metal (and indeed the fame muft
be obferved in the future procefs of polifhing) that no
kind of preffure is ufed that may endanger the bending,
or irregularly grinding them; they fhould therefore be
held with a light hand, and loofely between the fingers,
and the motion given fhould be in a horizontal direction,
with no more preffure than their own dead weight.

Having now finifhed the metal on the hones, and ren-
dered it both in point of figure and furface fit for the

<div align="right">laft</div>

last and most essential process, *viz*. that of polishing, I will describe it in the best manner I can; though many little circumstances which will be unavoidably omitted (and which at the same time are frequently essential to the success of a mechanic process) can only be supplied by actual experience.

The polishing the speculum is the most difficult and essential part of the whole process; for every experienced workman knows, to his vexation, that the most trifling error here will be sufficient to spoil the figure of his metal, and render all his preceding caution useless. I have, however, discovered a method which I shall explain, not only of giving the metal a parabolic figure, but also of recovering it when it happens to be injured; both to be effected in the act of polishing, and the former as certainly as the spherical figure is given upon the hones. Indeed, if we consider rightly, polishing will be perceived to be but a kind of grinding with a finer order of strokes, and with a powder infinitely finer than was before used in what is commonly called the grinding. But before I describe this method, which was the result of many years experience, I will take the liberty of making some few strictures on that of Mess. HADLEY and MOLYNEUX, which is followed by the generality of workmen.

First

Firſt, then, the tool itſelf uſed by them for poliſh-
ing the metal, is formed with infinite difficulty. The
firſt deſcribed poliſher is directed to be made by covering
the tool with ſarcenet, which is to be ſaturated with a
ſolution of pitch in ſpirit of wine, by ſucceſſive applica-
tions of it with a bruſh, till it is covered, and by the eva-
poration of the ſpirit of wine filled with this extract of
pitch; the ſurface is then to be worked down and finiſhed
with the bruiſer. This is all very eaſy in imagination;
but whoever has uſed this method (which I have myſelf
unſucceſsfully ſeveral times) muſt have found it attended
with infinite labour, and at laſt the buſineſs done in a
very unſatisfactory manner; for the pitch by this proceſs
will be deprived of an eſſential part of its compoſition.
The ſpirit of wine diſſolves none but the reſinous parts
of its ſubſtance, which is hard and untractable; and if
you uſe ſoap or ſpirit of wine to ſoften or diſſolve it, it
will equally affect the whole ſurface, the lower as well as
higher parts of it. And ſuppoſe that with infinite labour
with the bruiſer, it is at laſt reduced to a fine uniform
ſurface, it is neverthelefs too hard ever to give a good
poliſh with that luſtre which is always ſeen in Mr.
SHORT's, and indeed all other good metals. Nor will it
give a good ſpherical figure; for a perfect ſphere is
formed, as I obſerved before, by that intimate accommo-
dation

dation arifing from the wear and yielding of both tool and metal; whereas in this method, there is fuch a ftubbornnefs in the polifher, that the figure of the metal, good or bad, muft depend upon the truth of the former, which is very feldom perfect.

If the polifher be made in the fecond manner propofed, by ftraining the pitch through an outer covering, which is afterwards to be ftripped off, the fuperficies of pitch and farcenet is fo very thin, that the putty, working into them, forms a furface hard and untractable, fo that it is impoffible to give the fpeculum a fine polifh. Accordingly all thofe metals which are wrought that way have an order of fcratches inftead of polifh, difcovering itfelf by a greyifh vifible furface. Befides, fuppofing this tool perfectly finifhed, and anfwering its purpofe ever fo well, it is impoffible it can produce in the fpeculum any other than a fpherical figure; and indeed nothing elfe is expected from this method, as very evidently appears by the experiment recommended to afcertain the truth of the figure. You are directed to place a fmall luminous object in the center of the fphere of which the metal is a fegment, and then having adjufted an eye-glafs at the diftance of its own focal length from the object, and fo fituated that the image of the object formed by the fpeculum may be vifible to the eye, you are to judge of the

3

perfect

perfect figure of the metal by the ſharpneſs and diſtinct-neſs with which the image appears. From hence it is very evident, that as the object and image are both diſtant from the metal by exactly its radius, nothing but a true ſpherical figure of the ſpeculum can produce a ſharp diſtinct image; and that the image could not be diſtinct if the figure of the ſpeculum were parabolic. Conſe-quently, if the ſame ſpeculum uſed in a teleſcope were to receive parallel rays, there would neceſſarily be a con-ſiderable aberration produced, and a conſequent imper-fection in the image. Accordingly, there never was a good teleſcope made in this manner; for if the number of degrees, or the portion of the ſphere of which the great metal is a part, were as conſiderable as it ought to be, or as great as Mr. SHORT allowed in his metal, the inſtrument would bear but a very low charge, unleſs a great part of the circumference of the metal were cut off by an aperture, and the ill effects of the aberration by that means in ſome meaſure prevented.

If ever a finiſhed metal turned out without this defect, and has been found perfectly ſharp and diſtinct, it muſt have been owing to an accidental parabolic tendency, no ways the natural reſult of the proceſs, and therefore quite unexpected, and moſt probably unknown, to the work-man.

Without

Without enlarging, therefore, on the difficulty of the above procefs, and the impoffibility of giving the fpeculum the correctnefs and the kind of figure effentially neceffary to a good telefcope, I will defcribe (by way of introduction to the fucceeding directions) the fteps by which I was led to a certain and eafy method of giving a proper and correct parabolic figure to the metal, even though it came off imperfect from the hones, and an exquifite polifh at the fame time.

Having made many efforts in the former method, which by no means pleafed me for the reafons above-mentioned; and having obferved, from fome of Mr. SHORT's telefcopes which fell into my hands, that the high luftre of the polifh could never have been produced in the manner above defcribed, but by fome fofter and more tender fubftance; and at the fame time recollecting, that Sir ISAAC NEWTON had given an account in his Optics of his having finifhed fome metals, and confiderably mended the object glafs of a refractor, by working both upon a tool whofe furface had been covered with common pitch about the thicknefs of a groat; reflecting, I fay, upon thefe matters (coarfe and uncertain as this method appeared at firft fight) I was determined to try whether I could not get rid of my embarraffment, by a mode of operation fomewhat fimilar. Accordingly, fhortening

Dr. SMITH's proceſs, I made a ſet of tools in the manner
before deſcribed, except that I was obliged to make ſome
ſubſequent alteration in the poliſher which I ſhall pre-
ſently deſcribe.   Having given a good ſpherical figure
to the braſs tool and the bruiſer, and likewiſe to the
metal upon the hones, and made the braſs convex tool ſo
hot as juſt not to hurt the finger, I tied a lump of com-
mon pitch (which ſhould be neither too hard nor too
ſoft) in a rag, and holding it in a pair of tongs over a ſtill
fire where there was no riſing duſt, till it was ready to
ſtrain through the linen, I cauſed it to drop upon the
ſeveral parts of the convex tool, till I ſuppoſed it would
cover the whole ſurface about double the thickneſs of a
ſhilling; then ſpreading the pitch as equally as I could, I
ſuffered the poliſher (by which name I ſhall for the
future call this tool) to grow quite cold.   I then warmed
the bruiſer ſo hot as almoſt to burn my fingers, and hav-
ing fixed it to the bench with its face upwards, I ſud-
denly placed the poliſher upon it, and quickly ſlid it off;
by this means rendering the ſurface of the pitch more
equal.   The pitch is then to be wiped off from the bruiſer
with a little tow; and by touching the ſurface with a
tallow candle, and wiping it a ſecond time, it will be then
perfectly clean and fit for a ſecond proceſs of the ſame
ſort, which muſt again be performed as quickly as poſſible;

<div align="right">and</div>

and this is ordinarily fufficient to give a general figure to the furface of the pitch. The bruifer and polifher are then fuffered to grow perfectly cold, when the pitch, confidering what has been taken off, will be about the thicknefs of a fhilling.

It is however here neceffary to obferve, that the pitch fhould be neither very hard and refinous, nor too foft; if the former, it will be fo untractable as not to work kindly; and if too foft, it will in working alter its figure fafter than the metal, and too readily fit itfelf to the irregularity of its figure, if it have any. When both tools were perfectly cold, I gave the polifher a gentle warmth, and then fixed the bruifer to the block with its face upwards; and (having with a large camel's-hair brufh fpread over the face of the polifher a little water and foap, to prevent fticking) with fhort, ftraight, and round ftrokes I worked it upon the bruifer, every now and then adding a little more water and foap, till the pitch upon the polifher had a fine furface, and the true form of the bruifer; and this I continued to do till they both grew perfectly cold together: in this manner the polifher was perfectly formed in about a quarter of an hour. But here a difficulty arofe: when I begun to polifh the metal, I found that the edge of the hole in the metal collected the pitch towards the middle of the polifher; and

though

though in this method of working I could give an exquiſite poliſh, as the putty lodged itſelf in the pitch exceedingly well, yet the figure of the metal was injured in the middle, nor did indeed the work go on with that equability which is the inſeparable attendant on a good figure. In order to obviate this difficulty, I caſt ſome metals with a continued face, the holes not going quite through, within perhaps the thickneſs of a ſix-pence. I finiſhed two or three metals of this ſort, and the work promiſed and went on very well; but when I came to open the holes, which I did with the utmoſt caution, I found the metals ſhort of perfection; which I attributed to an alteration of the figure from the removal of even that ſmall portion of metal after the ſpeculum had been finiſhed. This I do ſuppoſe was in ſome meaſure the reaſon why I ſpoiled a very diſtinct and perfect two-foot metal, which bore a charge of two hundred times, only by opening the ſharp part of the edge of the hole, becauſe I thought it bounded the field: ſo eſſentially neceſſary is an exquiſite correctneſs of figure in the ſpeculum of a perfect reflector.

This experiment not ſucceeding, inſtead of caſting the metal without a hole, I made one quite through the middle of the poliſher, a little leſs than that in the ſpeculum. This perfectly anſwered the purpoſe; no more

incon-

inconvenience arofe from the gathering of the pitch (for it had now no greater tendency to collect at the center than the fides) and I finifhed feveral metals fucceffively, excellent both in point of figure and polifh ; one of thofe of two inches diameter and 7, 5 focal length, bore a charge of fixty times and upwards, which when mounted in a telefcope I gave to my brother. This telefcope underwent Mr. SHORT's examination, who was pleafed to remark only, that he thought he had made one more diftinct.

I muft obferve, that in this method of working the polifhing goes on in an agreeable, uniform, and fmooth manner; and that the fmall degree of yielding in the pitch (which is actually not more than the wearing of the metal) produces that mutual accommodation of furfaces fo neceffary to a true figure. In the beginning of the polifh, and indeed for fome time during the progrefs of it (always remembering now and then to move the metal round its axis) I worked round and round, not far from and always equally diftant from the center, except that every time, previous to the fhifting the metal on its axis, I ufed a crofs ftroke or two; and when the polifh was nearly compleated, I moftly ufed crofs ftrokes, giving a round ftroke or two likewife every time I turned the metal on its axis. I obferved in this method of working, that the metal always polifhed fafteft in the middle; infomuch,

infomuch, that half or two-thirds of it would be com-
pletely polifhed when the circumference of it was fcarcely
touched by the tool. Obferving this in fome of the firft
metals, and not confidering that this way of polifhing
was in fact a fpecies of grinding, and as perfect as that
upon the hones, I went on reluctantly with the work,
almoft defpairing of being able to produce a good
figure. However, I always found myfelf agreeably
deceived; for when the polifh was extended to the
edge, or within the tenth of an inch of it, I almoft
conftantly found the figure good, and the performance
of the metal very diftinct. But this fame circumftance of
apparent defect in the metals, was in fact that to which
their perfection was owing; for they all, contrary to my
expectation, turned out parabolic. However I did not
for a great while know any certain way of giving that
degree of parabolic tendency which was juft neceffary,
and which will be defcribed hereafter. It was a long
time before I got rid of my prejudice againft this appa-
rent imperfection in the procefs, or could reconcile my-
felf to the irregular manner in which the polifh pro-
ceeded; for I looked upon it as a certain fource of error,
and notwithftanding I faw it eventually fucceed, yet
whenever I chanced to find that a metal, when firft ap-
plied to the polifher, took the polifh equally all over,

and

and confequently the whole bufinefs did not take up above ten minutes; under thofe circumftances, I fay, I always ufed to pleafe myfelf with the expectation of a correct figure, at leaft as much fo as the metal had received from the hones, where the furface was but juft and equally taken off by the putty; but in this I conftantly found myfelf deceived, and the metal turned out good for nothing. In fhort, at this time, though I fpeculatively knew that a parabolic figure was neceffary to a perfect image, I yet confidered it as of little practical confequence.

From the foregoing experiments, and a number of fucceeding trials, I at length difcovered a certain way of giving a correct parabolic figure, and an exquifite polifh at the fame time. This, which I have ftrong reafons to believe was Mr. SHORT's method, I will now defcribe in as few words as I can.

How to polifh the fpeculum.

It is firft neceffary to obferve, that, in order to avoid the detrimental intrufions of any particles of emery, it would not be right to polifh in the fame room where the metal and tools were ground, nor in the fame cloaths which were worn in the former procefs; at leaft it would

be

be neceffary to keep the bench quite wet, to prevent any duft from rifing.

Having then made the polifher by coating the brafs convex tool equally with pitch, which we fuppofe fmoothed and finifhed with the brafs tool in the manner before defcribed, and which is a very eafy procefs, the whole operation is begun and finifhed in the following manner.

The leaden weight or handle upon the back of the metal fhould be divided into eight parts, by fo many deep ftrokes of a graver upon the upper furface of the lead, marking each ftroke with the numbers 1, 2, 3, 4, and fo on, that the turns of the metal in the hand may be known to be uniform and regular.

To prevent any mifchief from coarfe particles of putty, I always wafh it immediately before ufing. In order to this, put about half an ounce of putty into an ounce phial, and fill it two-thirds with water; then having fhaken the whole, let the putty fubfide, and ftop the bottle with a cork.

In a tea-cup with a little water, there fhould be a full-fized camel's-hair brufh, and a piece of dry clean foap in a galley-pot: a foft piece of fpunge will alfo be neceffary. Thefe, as well as the metal bruifer and polifher, fhould be conftantly covered from duft.

The

The polisher being fixed down, and the camel's-hair brush, being first wetted and rubbed a little over the soap, let every part of the tool be brushed over therewith; then work the bruiser with short, straight, and round strokes, lightly upon the tool, and continue to do so, now and then turning it, till the polisher have a good face, and be fit for the metal. Then having shaken up the putty in the phial, and touched the polisher in five or six places with the cork wetted with that and the water, place the bruiser upon the tool, and give a few strokes upon the putty to rub down any gritty particles; after which, having removed it, work the metal lightly upon the polisher round and round, carrying the edges of the speculum, however, not quite half an inch over the edge of the tool, and now and then with a cross stroke.

The first putty, and indeed all the succeeding applications of it, should be wrought with a considerable while; for if time be not given for the putty to bed itself in the pitch, and any quantity of it lie loose upon the polisher, it will accumulate into knobs, which will injure the figure of the metal: and therefore as often as ever such knobs arise, they must be carefully scraped off with the point of a penknife, and the loose stuff taken away with the brush. After the putty is well wrought into the pitch, some more may be added in the same manner, but

never much at a time, and always remembering to work upon it firſt with the bruiſer, for fear any gritty particles may find their way upon the poliſher. If the bruiſer be apt to ſtick, and do not ſlide ſmoothly upon the pitch, the ſurface of either tool may be occaſionally bruſhed over with the ſoap and water, but it muſt be remembered that the wet bruſh muſt be but lightly rubbed upon the ſoap.

In the beginning of this proceſs little effect is produced, and the metal does not ſeem to poliſh faſt, in ſome meaſure owing to its taking the poliſh in the middle, and perhaps becauſe neither that nor the bruiſer move evenly upon the poliſher: but a little perſeverance will bring the whole into a good temper of working; and, when the pitch is well defended by the coating of the putty, the proceſs will advance apace, and the former acquiring poſſibly ſome little warmth, the metal moves more agreeably over it, with an uniform and regular friction. All this while the metal muſt have no more preſſure than that which it derives from its own weight and that of the handle; and the poliſher muſt never be ſuffered to grow dry, but, as often as it has any tendency to do ſo, the edges of it muſt be moiſtened with the hair-pencil; and now and then, even when freſh putty is not laid on, the ſurface of the poliſher ſhould be touched with the bruſh to keep it moiſt.

4

When

When the polifh of the metal nearly reaches the edge (for it always, as I faid before, begins in the middle) you muft alter your method of working; for now the round ftrokes muft be gradually altered for the fhort and ftraight ones.   Suppofing then you are juft beginning to alter them; after having put on frefh putty, and gently rubbed it with two or three ftrokes of the bruifer, you place the metal on the tool, and after a ftroke or two round and round, give it a few forward and back-ward, and from fide to fide, but with the edges very little over the tool; then having turned the metal one-eighth round in your hand, and having moved yourfelf as much round the block (which muft be remembered throughout the whole procefs) you go on again with a ftroke or two round, to lead you only to the crofs ftrokes, which are now to be principally ufed, and with more boldnefs. After this has been done fome time, the metal will begin to move ftiffly as the friction now increafes, and the fpeculum polifhes very beautifully and faft; and the whole furface of the polifhing tool will be equally covered over with a fine metallic bronze. The tool even now muft not be fuffered to become dry; a fingle round ftroke in each of your ftations and turnings of the metal will be fufficient, and the reft muft all be crofs ones, for we are completing a circular figure. You muft now be very

diligent,

diligent, for the polisher drying, and the friction increasing very fast, the business of the spherical figure is nearly at an end. As the metal wears much, its surface must be now and then cleaned, with a piece of shammy leather, from the black stuff which collects upon it; and the polisher likewise from the same matter, with a soft piece of wet sponge. You will now be able to judge of the perfect spherical figure of the metal and tool, when there is a perfect correspondence between the surfaces, by the fine equable feel there is in working, which is totally free from all jerks and inequalities. Having proceeded thus far, you may put the last finishing to this figure of the metal by bold cross strokes, only three or four in the directions of each of the eight diameters, turning the metal at the same time: this must be done quickly, for it ought, in this part of the process particularly, to be remembered, that, if you permit the tool to grow quite dry, you will never be able, with all your force, to separate that and the metal, without destroying the polisher by heat.

The metal has now a beautiful polish and a true spherical figure, but will by no means make a sharp distinct image in the telescope: for the speculum (if it be tried in the manner hereafter recommended) will not be found to make parallel rays converge without great aberration;

aberration; indeed the deviation will be fo great, as to be very fenfibly perceived by a great indiftinctnefs in the image.

How to give the parabolic figure to the metal.

In order then to give the fpeculum the laft and finifhing figure, which is done by a few ftrokes, it muft be particularly remarked, that by working the metal round and round, the fphere of the polifher by this means growing lefs, it wears fafteft in the middle: and as a fegment of a fphere may become parabolic, by opening the extremes gradually from within outwards, fo it may be equally well done by increafing the curvature in the middle, in a certain ratio, from without inwards.

Suppofing then the metal to be now truly fpherical, ftop the hole in the polifher, by forcing a cork into it underneath, about an inch, fo that it do not reach quite to the furface; and having wafhed off any mud that may be on the furface of the tool with a wet foft piece of fponge, whilft the furface of it is a little moift, place the center of the metal upon the middle of the polifher; then having, with the wet brufh, lodged as much water round the edge of the metal as the projecting edge will hold,

hold, fill the hole of the metal and its handle with water, to prevent the evaporation of the moiſture, and the conſequent adheſion between the ſpeculum and poliſher, and let the whole reſt in this ſtate two or three hours: this will produce an intimate contact between the two, and by parting, with any degree of warmth they may have acquired by the vicinity of the operator, they will grow perfectly cold together.

By this time you may puſh out the cork from the poliſher, to diſcharge the water, and give the metal the parabolic figure in the following manner.

Move the metal gently and ſlowly at firſt, a very little round the centre of the poliſher (indeed after this reſt it will move ſtiffly) then increaſing by degrees the diameter of theſe ſtrokes, and turning the metal frequently round its axis, give it a larger circular motion, and this without any preſſure but its own weight, and holding it looſely between the fingers: this manner of working may ſafely be continued about two minutes, moving yourſelf as uſual round the block, and carrying the round ſtrokes in their increaſed and largeſt ſtate, not more than will move the edge of the metal half an inch or five-eighths over the tool. The ſpeculum muſt not all this while be taken off from the poliſher; and conſequently no freſh putty can be added.

added. It will not be fafe to continue this motion longer than the time above-mentioned; for if the parabolic tendency be carried the leaft too far, it will be impoffible to recover a true figure of that kind but by going through the whole procefs for the fpherical one in the manner before defcribed, by the crofs ftrokes upon the polifher, which takes a great deal of time. However, when there is occafion, it may be done; and I have myfelf feveral times recovered the circular figure, when I had inadvertently gone too far with the parabolic; and ultimately finifhed the metal on the polifher without the ufe of the hones.

To try the true figure of the metal.

It will now be proper to try the figure of the fpeculum, and that is always beft done by placing it in the telefcope it is intended for. In order to this, I ufe the inftrument as a kind of microfcope, placing the object, however, at fuch a diftance that the rays may be nearly parallel. At about twenty yards a watch-paper, or fome fuch object, on which there are fome very fine hair ftrokes of a graver, is fixed up. The lead muft be then taken off from the back of the fpeculum; which is beft done by placing the edge of a knife at the junction of the lead and metal, when, by ftriking the back of it with a

<div align="right">flight</div>

flight blow, the pitch immediately feparates, and the handle drops off; the remaining pitch may be fcraped off with a knife, taking care that none of the duft ftick to the polifhed face of the metal.

Having placed the fpeculum in the cell of the tube, and directed the inftrument to the object, make an annular kind of diaphragm with card-paper, fo as to cover a circular portion of the middle part of the metal between the hole and the circumference, equal in breadth to about an eighth part of the diameter of the fpeculum: this paper ring fhould be fixed in the mouth of the telefcope, and remain fo during the whole experiment, for the part of the metal covered by it is fuppofed to be perfect, and therefore unemployed.

There muft likewife be two other circular pieces of card-paper cut out, of fuch fizes, that one may cover the center of the metal by completely filling the hole in the laft defcribed annular piece; and the other, fuch a round piece as fhall exactly fit into the tube, and fo broad as that the inner edge may juft touch the outward circumference of the middle annular piece. It would be convenient to have thefe two laft pieces fo fixed to an axis that they may be put in their places, or removed from thence fo eafily as not to difplace or fhake the

3   inftrument.

inftrument. All thefe pieces therefore together will completely fhut up the mouth of the telefcope.

Let the round piece which covers the center of the metal, or that which has no hole in it, be removed; and, by a nice adjuftment of the fcrew, let the image (which is now formed by the center of the mirror) be made as fharp and diftinct as poffible. This being done, every thing elfe remaining at reft, replace the central piece, and remove the outfide annular one, by which means the circumference only of the fpeculum will be expofed, and the image now formed will be from the rays reflected from the outfide of the metal. If there be no occafion to move the fcrew and little metal, and the two images formed by thefe two portions of the metal be perfectly fharp and equally diftinct, the fpeculum is perfect, and of the true parabolic curve; or at leaft the errors of the great and little fpeculum, if there be any, are corrected by each other.

If, on the contrary, under the laft circumftance, the image from the outfide of the metal fhould not be diftinct, and it fhould become neceffary, in order to make it fo, that the little fpeculum be brought nearer, it is plain that the metal is not yet brought to the parabolic figure; but if, on the other hand, in order to procure diftinctnefs, you be obliged to move the little fpeculum farther off, then the figure of the great fpeculum has been carried beyond

the parabolic, and hath affumed an hyperbolic form. When the latter is the cafe, the circular figure of the metal muft be recovered (after having fixed on the handle with foft pitch) by bold crofs ftrokes upon the polifher, finifhing it again in the manner above defcribed. If the fpeculum be not yet brought to the parabolic form, it muft cautioufly have a few more round ftrokes upon the polifher; indeed a very few of them in the manner before defcribed make in effect a greater difference in the fpeculum than would be at firft imagined. If a metal of a true fpherical figure were to be tried in the above mentioned manner in the telefcope (which I have frequently done) the difference of the foci of the two fegments of the metal would be fo confiderable, as to require two or three turns of the fcrew to adjuft them; fo very great is the aberration of a fpherical figure of the fpeculum, and fo improper to procure that fharpnefs and precifion fo neceffary to a good reflecting telefcope.

This is by no means the cafe with the object glaffes of refractors; for befides that they are in fact never fo diftinct as well-finifhed reflectors, the apertures of them are fo exceedingly fmall, compared to the latter, and the number of degrees employed fo very fmall, that the inconvenience of a fpherical figure is not fo much perceived. Accordingly we obferve in the generality of

reflectors (whose specula, unless by accident, are always spherical) that the only true rays which form the distinct image arise from the middle of the metal: and unless the defect be remedied by a considerable aperture, which destroys much light, the false reflection from the inside of the metal produces a greyish kind of haziness, which is never seen in Mr. SHORT's or indeed in any good telescopes.

Supposing that the two foci of the different parts of the metal perfectly coincide, and that, by the union of them when the apertures are removed, the telescope shews the objects very sharp and distinct, you are not however even then to conclude that the instrument is not capable of farther improvement; for you will perceive a sensible difference in the sharpness of the image, under different positions of the great speculum with respect to the little one, by turning round the great metal in its cell, and opposing different parts of it to different parts of the little metal, correcting by this means the error of one by the other. This attempt should be persevered in for some time, turning round the great speculum about one-sixteenth at a time, and carefully observing the most distinct situation each time the eye-piece is screwed on: when, by trying and turning the great metal all round, the distinctest position is discovered, the upper part of the

metal

metal ſhould be marked with a black ſtroke, in order that it may always be lodged in the cell in the ſame poſition. This is the method Mr. SHORT always uſed; and the caution is of ſo much conſequence, that he thought it neceſſary to mention it very particularly in his printed directions for the uſe of the inſtrument.

And farther, Mr. SHORT frequently corrected the errors of the great by the little metal in another way. If the great ſpeculum did not anſwer quite well in the teleſcope, he cured that defect ſometimes by trying the effect of ſeveral metals ſucceſſively, by this means correcting the errors of one by the other; for in ſeveral of his teleſcopes which have paſſed through my hands, when the ſizes and powers have been the ſame, I have found that the great metals, though very diſtinct in their proper teleſcopes, yet have, when taken out and changed from one to the other, ſpoiled both teleſcopes, rendering them exceedingly indiſtinct, which could ariſe from no other circumſtance. For this reaſon I ſuppoſe it was, that he kept, ready finiſhed, a great many large metals of the ſame focal length, ſo that, when he wanted to mount a teleſcope, he might from a great choice, be able to combine thoſe metals which ſuited each other beſt. I am ſtrongly inclined to believe this was the caſe, not only from the above obſervation, but becauſe

he

he shewed me himself a box of finished metals, in which I am sure there were a dozen and a half of the same focal length.

To return: a little use in working will make the whole of the process of grinding and polishing very easy and certain; for though I have endeavoured to be as particular as I can (I am almost afraid too much so) it is yet scarcely possible to supply a want of dexterity, arising from habit only, by the most laboured and minute description. And though the above account may appear irksome to the reader, as it lies cold before the eye, I am very sure, whoever attempts to make the instrument, will not complain of it as tediously particular.

I will, however, farther remark, that when the metal begins to move stiffly upon the polisher, and particularly when the figure is almost brought to the parabolic form, it will be necessary to fix the elbows against the sides, in order to give momentum and equability to the motion of the hand by that of the whole body.

The same polisher will serve for several metals, if it be somewhat warmed when you begin to use it.

There is another circumstance, and a material one too, which must not be omitted; it is this. For the very same reason that the pitch should not be too hard or soft, the work will not proceed well in the heat of summer,

or

or the cold of winter: in the latter, it may be poſſible to remedy the defect by having the room warmed with a ſtove; and in the ſummer, the other inconvenience may perhaps be avoided by uſing a harder kind of pitch; but I much doubt in either caſe whether the work will go on ſo kindly: I have myſelf always wrought in ſpring and autumn.

The proceſs of poliſhing, and indeed grinding upon the hones, will not go on ſo well if it be not continued uninterruptedly from beginning to end; for if the work of either kind be left but for a quarter of an hour, and you then return to it again, it will be ſome time before the tool and metal can get into a kindly way of working; and till they do, you are hurting what was done before.

I have all along ſuppoſed that the metal we have been working was about four inches diameter: if it be either larger or ſmaller, the ſizes of the hones, bruiſer, and poliſher, muſt be proportionably different. I never find any ill conſequence ariſing from the different expanſion from heat and cold in any of the tools, though they be made of different metals and ſubſtances, unleſs the inconvenience, occaſioned by the interruption before hinted at, be thought to reſult from thence; for the alteration produced in the ſurface of the ſpeculum, both by grinding and poliſhing, is ſo much quicker than any that can be ſuppoſed to ariſe

from

from the former caufe, that it is never attended with any practical confequence.

Magnifying very minute objects, and particularly reading at a diftance, have been generally confidered as the fureft tefts of the goodnefs of a telefcope; and indeed when the page is placed at a great diftance, fo that the letters fubtend but a very fmall angle at the eye, if then they appear with great precifion and fharpnefs, it is moft probable that the inftrument is a good one. But we are, neverthelefs, fometimes apt to be deceived by this method; nor is it always poffible to determine upon the different merits of two inftruments of equal power, by this mode of examination; for when the letters are removed to the utmoft extent of the powers of the two inftruments, the eye is apt to be prejudiced by the imagination. If two or three words can be here and there made out, all the reft are gueffed at by the fenfe; infomuch that an obferver, zealous for the honour of his inftrument, is very apt to deceive himfelf in fpite of his intentions. The furer teft is by figures, where you can procure no aid from this fort of deception. In order to examine my reflecting telefcopes, I made upon a piece of copper and on a black ground, fix lines confifting of about twelve pieces of gold figures, and each line of figures differing in magnitude, from the fmalleft that could be diftinctly made to thofe

of

of about two-tenths of an inch long; moreover, the figures in the feveral lines were differently difpofed, and the fum of each line alfo differed. It is evident that by this method all guefs is precluded; and that of two inftruments, of the fame powers, that which can make out the leaft order of figures, which will be known by the fum, is the beft telefcope. Such a plate I caufed to be fixed up for experiments againft the top of a fteeple, about three hun-dred yards North of my houfe; and it will ferve to give fome idea of the diftinctnefs with which very fmall figures could be made out at that diftance, by faying, that in a clear ftate of the air, and with the Sun behind me, with a telefcope of eighteen inches focal length, which Count BRUHL did me the honour to accept and now has in his poffeffion, I have feen the legs of a fmall fly, and the fhadows of them, with great precifion and exactnefs.

I cannot conclude without indulging myfelf in an ob-fervation on the amazing fagacity of Sir ISAAC NEWTON in every fubject upon which he thought fit to employ his attention. It was he who firft propofed, and indeed practifed, the polifhing with pitch; a fubftance which at firft fight perhaps every one but himfelf would have thought very improper, from its foftnefs, to produce that correctnefs of figure fo neceffary upon thefe occafions; and yet I do believe, that it is the only fubftance in nature

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that is perfectly well calculated for the purpose; for at the fame time that it is foft enough to fuffer the putty to lodge very freely on its furface, and for that reafon to give a moft tender and delicate polifh; it is likewife totally inelaftic, and therefore never, from that princi- ple, fuffers any alteration in the figure you give it. If the firft makers of the inftrument, therefore, had given proper credit to, or had fimply followed the hint Sir ISAAC gave, it would have faved them infinite trouble, and they would have produced much better inftruments; but the pretended refinement, of drawing a tincture from pitch with fpirits of wine, affords you only the re- finous, hard, and untractable part of the pitch, divefted of all that part of its original fubftance which is neceffary to give it that accommodating pliability in which its excellence confifts.

It is needlefs to fwell this account with a detail of the procefs for polifhing the little fpeculum, as it muft be conducted in the fame manner which has been already defcribed in that of the large one; only obferving, that as the little metal has an uninterrupted face, without a hole, fo there is no occafion for one in the polifher; and likewife that, as a fpherical figure is all that need here be practically attempted, fo the difficulty in finifhing is infi- nitely fhort of that of the other.

As it is always neceſſary to ſolder to the back of the little ſpeculum a piece of braſs, as a fixture for the ſcrew to adjuſt its axis, I ſhall juſt hint a ſafe and neat method of doing it, which may be very uſeful to the optical or mathematical inſtrument-maker upon other occaſions. Having cleaned the parts to be ſoldered very well, cut out a piece of tin-foil the exact ſize of them; then dip a feather into a pretty ſtrong ſolution of *ſal ammoniac* in water, and rub it over the ſurfaces to be ſoldered; after which place the tin-foil between them as faſt as you can (for the air will quickly corrode their ſurfaces ſo as to prevent the ſolder taking) and give the whole a gradual and ſufficient heat to melt the tin. If the joints to be ſoldered have been made very flat, they will not be thicker than a hair: though the ſurfaces be ever ſo extenſive, the ſoldering may be conducted in the ſame manner, only that care muſt be taken, by general preſſure, to keep them cloſe together. In this manner, for inſtance, a ſilver graduated plate may be ſoldered on to the braſs limb of a quadrant, ſo as not to be diſcernable by any thing but the different colour of the metals. This method was communicated to me by the late Mr. JACKSON, who during his life kept it a ſecret, as he uſed it in the conſtruction of his quadrants, and is, I believe, not as yet known to any workman.

In

In the annexed plate are figured the fhape of the leaden tool for rough-grinding; the hones; and the apparatus to be applied to the mouth of the telefcope, to afcertain the true figure of the fpeculum.

P O S T S C R I P T.

It was fome time after I had written the above account that I faw Mr. SHORT's method of polifhing object glaffes for refracting telefcopes, which is publifhed in the Tranfactions. By that paper I find that what I before ftrongly fufpected is really the cafe, *viz.* that he knew how well pitch was calculated for purpofes of this kind. Only it may be remarked, that as glafs is much harder, polifhes much flower, and confequently does not wear away and alter its figure fo foon as the metal of which the fpeculum is made; and as at the fame time (on account of the very fmall apertures allowed to telefcopes of this fort) nothing more than a fpherical figure is propofed; he is therefore obliged to ufe pitch in a hard, friable, and ftubborn ftate: whereas, confidering the delicate fubftance of the metal fpeculum, and the figure intended to be given to it, the foft pitch of the common fort, by fuffering the putty to bed itfelf in its fubftance,

Y y 2 produces

produces the moſt beautiful poliſh; and by its pliability is better calculated for that mutual accommodation between poliſher and metal, ſo neceſſary to the figure propoſed.

EXPLANATION OF THE FIGURES.

Fig. 1. The grinder for working off the rough face of the metal; the black ſtrokes repreſent deep grooves. made with a graver.

Fig. 2. The bed of hones, which is to complete the ſpherical figure of the ſpeculum, and to render its ſurface fit for the poliſher.

Fig. 3. An apparatus for examining the parabolic figure of the ſpeculum.

AA The mouth of the teleſcope, or edge of the great tube.

BB A thin piece of wood faſtened into, and fluſh with the end of the tube; to which is permanently fixed the annular piece of paſte-board cc, intended to cover, and to prevent the action of the correſponding part of the ſpeculum.

D. Ano-

Fig:1.
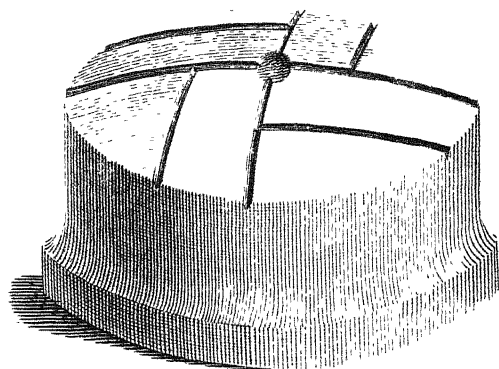
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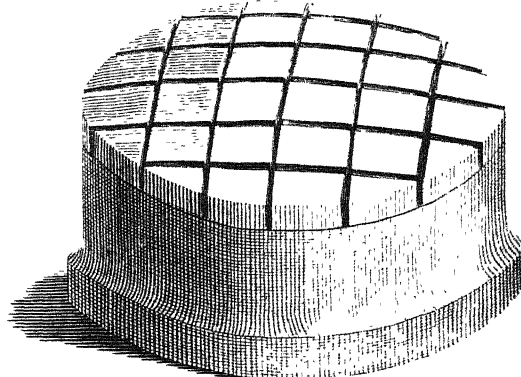
Fig:3.
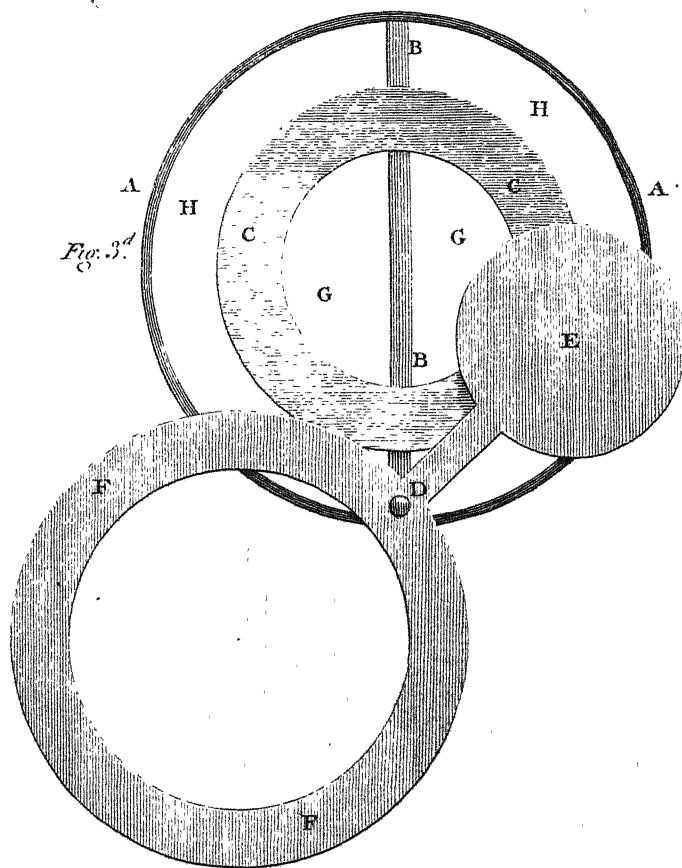
D Another piece of paste-board, fixed by a pin to the piece of wood BB, on which it turns as on a center; fo that the great annular opening HH may be fhut up by the ring FF, or the aperture GG by the imperforate piece E in fuch manner that, in the firft inftance, the reflexion may be from the center, and in the latter from the circumference, of the great fpeculum.
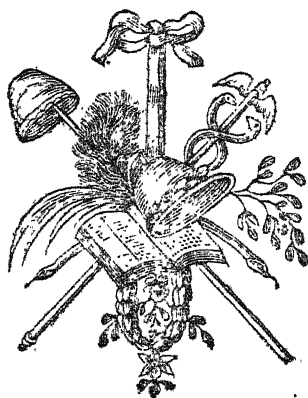
XVII. *Extract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in Rutland,* 1776. *By* Thomas Barker, *Esquire.* *Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read February 20, 1776.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest	Lowest.	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,65	28,88	29,27	42½	23	32½	42	10	26	2,511
	Aftern.				43	26	33	45	16	29	
Feb.	Morn.	29,50	28,24	28,89	44½	24	39	44	11	35	3,195
	Aftern				45	25	40	45½	27	41½	
Mar.	Morn.	29,95	28,50	29,46	53½	38½	44½	47½	27	38	1,518
	Aftern.				55	40	46	62½	37	48½	
Apr.	Morn.	29,94	29,00	29,58	54½	43	50	52	31	42½	0,887
	Aftern.				57½	44	51½	64½	42	55	
May	Morn.	29,99	28,73	29,57	62½	47	51½	58	38	47	1,627
	Aftern.				66	48	53	76	38	57	
June	Morn.	29,87	29,04	29,42	66	55½	59	62	48½	54	2,485
	Aftern.				69½	57½	61	75	54	64½	
July	Morn.	29,86	29,03	29,45	65	57	62	63½	53	58	1,850
	Aftern.				68½	59½	64	80	65	69½	
Aug.	Morn.	29,83	29,00	29,41	70	57	61	63½	46	54	5,200
	Aftern.				75	59	62½	82½	58½	66	
Sept.	Morn.	29,93	28,75	29,40	62	52½	57½	60	37	49½	2,452
	Aftern.				64	54½	58½	69	52½	61	
Oct.	Morn.	29,87	29,06	29,56	57½	48	52½	53	36	45½	2,061
	Aftern.				58	48	53½	62	48	54	
Nov.	Morn.	29,85	28,60	29,42	52	37	45½	54	26½	38½	2,823
	Aftern.				52	37	46	54	32½	44	
Dec.	Morn.	29,94	28,73	29,43	48½	34	42½	49	21½	37	1,233
	Aftern.				49	34	43	52	26½	40½	

27,842

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The year began wet, but there soon fell a greater quantity of snow than for several years past: we had perhaps the sharpest frost since 1740, and it was more intense at the latter part of it than at the beginning. The frost went away finely the beginning of February, and without much rain till the snow was almost gone; but a good deal of the middle of February was stormy and wet, and it was chiefly wet till about ten days in March, yet not cold. Then the season grew dry; the seed-time was fine, pleasant, growing warmer, and in the middle of April hot. There was great plenty of blossoms of all sorts, and the grass came on well; but the wheat, which had been left thin by the great frost and snow, was rather hurried on too fast. It grew colder at the end of April, and was cool and dry most part of May, with chiefly Northerly winds. During this time the wheat mended much, but rain began to be wanted, of which there came some in June, and brought on both corn and grass. The latter end of that month and July were only showery and hotter, so that the hay was got in well; and toward the end of July and beginning of August, the ground began to burn pretty much, when after some very hot days there came a great deal of wet.

The course of the seasons this year was I believe the same in all places. Dry spring and beginning of summer, showery June, drier July, very hot beginning of August,

5   and

and wet after; but in different proportions in various places. In fome, as Leicefterfhire and Northamptonfhire, the drought fo much prevailed that the ground was greatly burnt, and hay very fcarce; on the other hand here and in Huntingdonfhire, there were fo many refrefhing fhowers that we never were in want of grafs.

The beginning of harveft was wet, and the reft fhowery. I believe a little of the wheat might grow, but in general the grain was pretty well got in. It was a remarkably plentiful year for almoft all kinds of fruit; the crop of grain was pretty good, efpecially the barley; and there were great quantities of latter grafs and turnips. In the middle of September the weather grew fairer; it was a fine latter end of the year and wheat feed-time, without too much rain intermixed. As the winter came on, it was chiefly calm, and there was much cloudy or mifty weather, fcarce any frofty mornings till near the end of November; a fhort froft then and mild again; but towards the latter half of December it began to be more inclined to froft, and the year ended with a fharp one, and pretty deep fnow.

XVIII. *Extract of a Meteorological Journal for the Year* 1776, *kept at* Bristol, *by* Samuel Farr, *M. D.*

Read February 27, 1777.

Months.	Barometer.			
	Higheft.	Loweft.	Mean.	Viciffitude.
January	29,93	29,04	29,53	— 47- ½
February	29,74	28,66	29,21	+ 102-2
March	30,28	28,80	29,53	+ 110-2
April	30,28	29,26	29,85	+ 68-2
May	30,30	29,10	29,88	— 40- ½
June	30,20	29,38	29,88	— 39-1½
July	30,16	29,30	29,76	— 40-1½
Auguft	30,08	29,30	29,68	— 30- ½
September	30,20	20,17	29,72	— 98-4
October	30,18	29,60	29.87	+ 54-1
November	30,15	28,90	29,74	+ 65- ½
December	30,28	29,26	29,76	+ 32- ½

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— falling.

An abridged Table of the WINDS, &c. for BRISTOL, for the Year 1775.

	N	E	W	S	NW	SE	NE	SW	Rain.	Frosty Days.	Fair Days.	Thunder, &c,
Jan.	0	1	0	0	½	3	23½	3	3,993	25	6	
Feb.	0	0	0	1½	1	3½	2½	20½	5,538	1	5½	5. S.W.
Mar	0	1½	0	1½	1½	3	7½	16	1,643	2½	13½	
Apr.	1½	1	½	2	4	2	10½	8½	0,438	1	18	
May	2½	2	½	2½	4	1	11½	7	1,149	0	13½	
June	½	½	0	3	2	5	5½	13½	2,554	0	13½	13. S.
July	0	0	½	3	3	5	1	18½	2,332	0	13	5. S.E. 19. S.E.
Aug.	½	½	½	1	5	6½	3	14	4,747	1	11½	{ 6. N.W. 16. S.W. { 30. S.W.
Sept.	0	0	0	½	4	3	10	12½	3,270	5	15½	4. N.W. 8. N.W.
Oct	0	1½	0	1½	3½	4	13	7½	1,686	4	6	
Nov.	1	0	½	0	2	13	1½	12	2,283	5	10½	30. S.E.
Dec.	3½	½	½	½	3½	10½	2	10	1,422	9	11	1. S.E.
	9½	8½	3	17	34	59½	91½	143	31,055	53½	137½	

WEATHER FOR THE YEAR 1776.

January. It rained a little at firſt; but foon changed to fnow, which was fucceeded by a hard froſt that laſted till the next month.

February. After the thaw it became very wet and tempeſtuous.

March. Often froſty and fair, with now and then a confiderable ſtorm, as on the 8th and 9th.

3

April.

April. Stormy, although but little rain fell for about half this month; after the 20th it was quite fair.

May. After this it was ſtormy, with ſome rain on the 4th, 5th, 6th, 8th, 11th, 19th, and 20th, but not much; the reſt of this month was fair.

June. Very wet for 7 or 8 days, and on the 10th, 11th, and 12th, and alſo from the 14th to 17th; it was then fair to the 23d; rained then and on the 24th and 27th.

July. Very wet and cloudy, with thunder, &c. on the 5th and 19th; very few days fair till the 22d; after which it was in general fair this month.

Auguſt. Very fine for a few days; but afterwards it became wet and ſtormy till the 20th; after which it was in general fair.

September. Stormy for ſome days; fair on the 8th and 9th; froſty on the 10th, and then fair to the 15th; wet to the 18th; and then fair to the end.

October. Dry though cloudy, with a little rain for 7 or 8 days; after which it rained part of every day for as many more, and then became variable with ſome froſty nights.

November. Dry with ſome froſt and fog, but with little rain on the 9th and 11th, and for a fortnight after;

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it next became variable, but generally fair; it rained very hard on the 23d and on the 28th.

December. Wet for a few days and then cloudy, but without rain for the firſt week; it was then dry for a few days; it froze on the 16th, and ſnow fell on the 18th; the 19th was fair; it rained afterwards to the 23d, and ſnowed on the 24th; after which a froſt ſat in, and continued till the end.
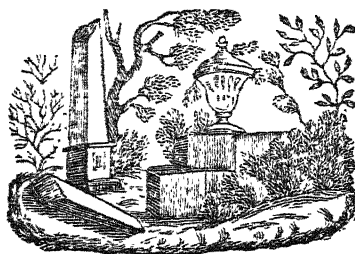
METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1776.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8 0	38,5	39,0	29,94		SE	1	Cloudy.
	2 0	41,0	40,0	29,82		S	1	Fine.
2	8 0	41,0	41,5	29,50	0,075	NNW	1	Rain.
	2 0	40,5	41,5	29,69		NNW	1	Cloudy.
3	8 0	36,0	39,5	30,09	0,065	NE	1	Cloudy.
	2 0	37,0	40,5	30,14		E	1	Fine.
4	8 0	39,0	38,5	29,77	0,031	SSE	1	Rain.
	2 0	44,0	40,0	29,50		S	2	Rain.
5	8 0	44,5	43,5	29,40	0,444	SW	1	Fair.
	2 0	48,5	45,5	29,48		WSW	1	Cloudy.
6	8 0	36,0	43,5	29,63		WSW	1	Fine.
	2 10	41,5	44,0	29,57		E by N	1	Fair.
7	8 30	33,0	39,5	29,245	0,153	ENE	2	Rain.
	2 0	31,0	38,0	29,35		ENE	2	Snow.
8	8 0	30,0	34,0	29,46	0,229	NE	1	Much fnow laft night.
	2 0	32,0	35,5	29,49		N	1	Fair.
9	8 0	30,0	34,5	29,63	0,079	N	1	Cloudy.
	2 0	28,5	33,5	29,67		ENE	1	Cloudy.
10	8 0	30,5	33,5	29,73		NNE	1	Cloudy.
	2 0	33,0	34,0	29,73		NW	1	Cloudy.
11	8 0	30,0	31,5	29,57		E	1	Cloudy.
	2 0	35,0	33,5	29,41		SE	1	Cloudy.
12	8 0	30,0	33,0	29,21	0,091	E	2	Much fnow and wind laft nt.
	2 0	32,0	34,0	29,21		ENE	2	Snow.
13	8 0	30,0	32,5	29,32		NE	2	Much fnow and wind laft nt.
	2 0	29,0	32,0	29,36		NE	2	Cloudy.
14	8 0	25,0	30,0	29,45		ENE	2	Snow.
	2 0	26,0	29,0	29,49		NE	2	Snow.
15	8 0	26,0	29,0	29,70		N	1	Cloudy.
	2 0	29,0	30,5	29,70		ENE	1	Snow.
16	8 0	25,5	29,0	29,71		NNE	1	Snow.
	2 0	30,5	30,5	29,70		NE	1	Snow.

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METEOROLOGICAL JOURNAL.

for January 1776.

Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
H	M.			Inches.	Inch.	Points.	Str.	
Jan.17. 8	0	28,0	29,5	29,74		NNE	1	Cloudy.
2	0	30,5	30,5	29,74		NNE	1	Cloudy.
18 8	0	33,0	32,0	29,87		ENE	1	Cloudy.
2	0	30,0	31,5	29,90		ENE	1	Cloudy.
19 8	0	27,0	30,5	29,90		NE	1	Snow.
2	0	27,0	31,0	29,85		NE	1	Fair.
20 8	0	23,5	29,0	29,70		N	1	Fair.
2	0	24,0	29,0	29,70		N	1	Fine.
21 8	0	22,0	27,0	29,62		SSW	1	Cloudy.
2	0	28,5	27,5	29,55		SSW	1	Fine.
22 8	0	30,5	28,5	29,41		E	1	Fair.
2	0	33,5	30,5	29,39		NE	1	Fine.
23 8	0	25,0	28,5	29,53		NE	1	Fair.
2	0	33,0	30,0	29,58		NNE	1	Snow.
24 8	0	30,0	30,0	29,81		ENE	0	Cloudy.
2	0	35,0	31,5	29,84		NE by E	1	Fair.
25 8	0	29,0	31,5	29,87		NE	1	Cloudy.
2	0	33,0	32,5	29,85		SE	1	Fair.
26 8	0	26,0	30,5	29,82		E	1	Cloudy.
2	0	26,0	30,5	29,82		SSE	2	Fine.
27 8	0	19,5	26,0	29,80		ENE	2	Snow.
2	0	20,5	25,5	29,79		ENE	2	Snow.
28 8	0	18,5	22,5	29,94		E	2	Snow.
2	0	22,0	23,0	30,00		E	2	Fine.
29 8	0	14,5	20,0	29,91		SE	1	Fine.
2	0	24,0	21,5	29,88		SE by E	1	Fair.
30 8	0	15,0	19,5	29,97		NE	1	Fair.
2	0	21,0	20,5	30,02		E	1	Fine.
31 8	0	13,5	19,5	30,09		NE	1	Fair.
2	0	23,5	20,5	30,07		E	1	Fine.

METEOROLOGICAL JOURNAL

for February 1776.

	Time. H. M.	Therm. without	Therm. within	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Feb. 1	8 0	14,5	19,0	29,97		NE	1	Fair.
	2 0	31,5	31,5	29,86		E	1	Cloudy.
2	8 0	37,0	28,5	29,78		NE	0	Cloudy.
	2 0	40,5	31,0	29,57		SE by E	1	Fair.
3	8 0	37,0	34,5	29,34	0,093	SSE	1	Fog.
	2 0	44,0	36,0	29,51		S by W	1	Fine.
4	8 20	44,0	38,5	29,22		SW	2	Cloudy.
	2 0	48,0	41,0	29,22		SW	1	Cloudy.
5	8 0	40,5	42,0	29,16	0,153	WSW	2	Fine.
	2 0	48,0	44,0	29,11		SWbyW	2	Fine.
6	8 0	37,0	42,5	28,98	0,221	WSW	2	Fine.
	2 0	45,0	44,0	29,02		WSW	2	Fine.
7	8 0	43,0	42,0	28,91		SWbyW	2	Fair.
	2 0	47,5	43,5	29,07		SW	2	Fair.
8	8 0	40,0	42,5	29,66	0,190	SW	1	Rain.
	2 0	49,5	44,5	29,62		SSW	2	Cloudy.
9	8 0	41,0	44,5	29,78	0,190	SSW	1	Fine.
	2 0	45,5	45,5	29,58		S	2	Cloudy.
10	8 0	42,0	46,0	28,99	0,524	SWbyW	2	Rain.
	2 0	48,0	47,0	28,97		SW	2	Fair.
11	8 0	42,5	45,0	28,84	0,275	SW	2	Fair.
	2 0	45,5	46,0	28,84		SSW	2	Rain.
12	8 0	34,0	42,0	29,10		SSW	1	Fair.
	2 0	46,0	44,0	29,24		WSW	1	Fine.
13	8 0	36,5	42,5	29,68		SW	1	Fine.
	2 0	47,0	44,0	29,71		WSW	1	Fine.
14	8 0	47,0	45,0	29,51	0,163	SW	2	Fair.
	2 0	51,0	48,0	29,53		S by W	1	Fine.
15	8 0	38,0	44,5	29,96		SSW	1	Fine.
	2 0	48,0	46,5	29,91		SSW	2	Fair.
16	8 0	42,0	46,0	29,75	0,085	SW	1	Fine.
	2 0	49,5	47,5	29,78		SW	1	Fair.

METEOROLOGICAL JOURNAL
for February 1776.

Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
H.	M			Inches	Inch.	Points.	Str.	
Feb. 17 8	0	36,0	43,0	29,68	0,021	ENE	1	Rainy.
2	0	43,5	42,5	29,53		E	1	Fair.
18 7	0	35,5	42,0	29,43	0,270	SSW	1	Fair.
2	0	39,5	42,0	29,25		NE	1	Cloudy.
19 7	0	38,5	42,0	29,34	0,140	SW	1	Rainy.
2	0	41,5	41,5	29,62		NW	1	Fair.
20 7	0	41,5	40,5	29,775	0,089	S	1	Rainy.
2	0	50,0	44,0	29,80		W by S	1	Fine.
21 7	0	49,5	45,5	29,58		WSW	2	Cloudy.
2	0	54,5	49,0	29.50		WSW	2	Fair.
22 7	0	36,5	44,0	29,62	0,412	SW	1	Fine.
2	0	45,0	45,0	29,41		SSE	3	Rain.
23 7	0	35,0	40,5	29,38	0,148	WSW	1	Fine.
2	0	45,5	44,0	29,50		WSW	2	Fair.
24 7	0	39,0	41,0	29,25	0,079	SSE	1	Rain.
2	0	46,0	48,0	29,10		WSW	1	Rain.
25 7	0	37,0	40,0	29,50	0,102	N	1	Cloudy.
2	0	43,0	41,5	29,55		SSW	1	Fair.
26 7	0	46,0	43,5	29,18		SSW	1	Cloudy.
2	0	53,0	46,5	29,13		S by W	2	Cloudy.
27 7	0	39,5	43,0	29,155	0,149	SSW	2	Cloudy.
2	0	48,0	45,0	29,26		WSW	2	Fair.
28 7	0	43,0	43,5	29,11	0,140	SSW	2	Fair,
2	0	47,0	45,5	29,17		SW	2	Fair.
29 7	0	38,0	43,0	29,35	0,066	SSW	1	Fine.
2	0	48,0	45,0	29,35		WSW	1	Fine.

METEOROLOGICAL JOURNAL

for March 1776.

Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
H. M.				Inches.	Inch.	Points.	Str.	
Mar. 1	7 0	32,0	42, 0	29,52	0,080	WSW	1	Fair.
	2 0	45,5	43,5	29,60		NE	1	Fine.
2	7 0	35,0	41,0	29,75		N	1	Fair.
	2 0	43,0	42,5	29,77		NNE	1	Fair.
3	7 0	30,5	37,0	29,78		SSW	1	Fine.
	2 0	44,0	40,5	29,67		SSW	1	Fair.
4	7 0	36,0	38,0	29,49	0,159	N	2	Cloudy.
	2 0	44,5	41,5	29,73		NNW	2	Fair.
5	7 0	44,0	42,5	29,81	0,058	SW	2	Fair.
	2 0	46,0	44,5	29,85		SW	2	Rain.
6	7 0	41,5	44,0	29,77	0,314	SE	1	Rain.
	2 0	43,5	44,5	29,58		E	1	Rain.
7	7 0	39,0	43,0	29,70	0,514	E	1	Cloudy.
	2 0	42,0	43,5	29,59		SSE	1	Rain.
8	7 0	40,0	45,0	29,25	0,229	SSW	1	Fine.
	2 0	49,0	47,0	29,18		SW	2	Fair.
9	7 0	43,0	45,5	28,98	0,065	S	2	Fair.
	2 0	50,0	49,0	28,99		SSW	2	Fair.
10	7 0	40,5	44,5	29,37	0,079	WSW	1	Fair.
	2 0	48,0	46,5	29,50		W	1	Fine.
11	7 0	35,0	41,0	29,72		NNW	1	Fine.
	2 0	46,0	44,0	29,86		NW	1	Fine.
12	7 0	41,5	42,0	29,95		SSW	1	Cloudy.
	2 0	49,0	45,0	29,99		SSW	1	Rain.
13	7 0	46,0	46,5	30,00	0,014	SSW	1	Fair.
	2 0	53,5	49,0	29,86		SSW	1	Fair.
14	7 0	41,5	45,5	29,83		N	1	Fair.
	2 0	49,0	47,0	29,94		N	1	Fine.
15	7 0	40,0	45,5	30,03		W	1	Fine.
	2 30	55,0	48,5	30,05		WSW	1	Cloudy.
16	7 0	43,0	48,0	30,19		N	1	Fine.
	2 0	49,5	49,0	30,21		N	1	Fine.

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METEOROLOGICAL JOURNAL
for March 1776.

	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch	Points.	Str.	
Mar. 17	7	0	38,0	46,0	30,30		S	1	Fine.
	2	0	53,5	48,5	30,30		WSW	1	Fine.
18	7	0	34,0	45,0	30,18		SW	1	Fine.
	2	0	53,5	48,0	30,13		SW	1	Fine.
19	7	0	47,0	48,0	30,06		SW	1	Cloudy.
	2	0	50,0	46,0	30,09		SW	1	Cloudy.
20	7	0	48,0	49,0	30,15		W	1	Cloudy.
	2	0	55,0	52,0	30,23		SSW	1	Cloudy.
21	7	0	43,5	49,5	30,23		E	1	Fine.
	2	0	59,5	53,5	30,20		SW	1	Fair.
22	7	0	49,0	52,0	29,96		SW	1	Fine.
	2	0	63,5	60,0	29,95		SSW	1	Fine.
23	7	0	46,5	49,5	29,94		NE	1	Fog.
	2	0	66,0	57,0	29,91		SW by S	1	Fine.
24	7	0	52,5	56,0	29,94		NNE	1	Fair.
	2	0	67,0	59,0	29,99		SE	1	Fine.
25	7	0	45,0	52,0	30,30		ENE	1	Fair.
	2	0	49,0	49,0	30,33		ENE	2	Fine.
26	7	0	37,0	42,0	30,38		NE	1	Fair.
	2	0	49,5	46,0	30,33		NE	1	Fine.
27	7	0	33,5	42,0	30,13		NNE	1	Fine.
	2	0	51,5	45,0	30,04		NNE	1	Fine.
28	7	0	41,5	44,5	30,01	0,029	NNE	1	Cloudy.
	2	0	46,0	46,0	30,03		NE	1	Cloudy.
29	7	0	40,0	44,5	30,11		N	1	Fair.
	2	0	47,5	46,0	30,20		NNE	1	Fair.
30	7	0	35,0	44,0	30,37		S	1	Fine.
	2	0	56,5	47,5	30,38		N by W	1	Fine.
31	7	30	44,5	47,5	30,43		WSW	1	Fair.
	2	0	60,5	51,5	30,42		WSW	1	Fine.

METE-

METEOROLOGICAL JOURNAL

for April 1776.

	Time. H. M.	Therm. without.	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Apr. 1	7 0	44,0	50,5	30,44		SSW	1	Fine.
	2 0	60,0	54,0	30,43		N	1	Fine.
2	7 0	48,0	53,0	30,37		NNE	1	Cloudy.
	2 0	59,0	54 5	30,34		NE	1	Fine.
3	7 0	42,5	51,5	30,34		SW	1	Fine.
	2 0	62,0	56,5	30,28		W	1	Fine.
4	7 0	45,0	51,0	30,22		N	1	Fine.
	2 0	54,0	53,5	30,22		N	1	Fine.
5	7 0	41,0	48,5	30,22		N	1	Fair.
	2 0	49,0	49,5	30,17		N	1	Fair.
6	7 0	40,5	46,5	30,145		N	1	Cloudy.
	2 0	47,0	48,0	30,11		SSW	1	Cloudy.
7	7 0	45 5	46,0	30,03		WNW	1	Fair.
	2 0	50,5	48,5	29,94		NW	1	Cloudy.
8	7 0	45,0	48,0	29,75		WNW	1	Fair.
	2 0	51,5	50,0	29,57		NW	2	Fine.
9	7 0	39,0	43,5	29,71	0,065	NW	2	Fair.
	2 0	39,0	45,0	29,86		NWbyN	2	Cloudy.
10	7 0	34,5	41,0	30,13	0,052	N	1	Fine.
	2 0	48,5	44,0	30,16		NNE	2	Fine.
11	7 0	44,5	44,0	30,21		NNE	1	Cloudy.
	2 0	54,0	48,0	30,21		N	1	Fair.
12	7 0	44,0	46,0	30,17		NW	1	Fair.
	2 0	53,5	49,5	30,07		WNW	1	Fair.
13	7 0	45,0	48,5	29,85		SW	1	Fair.
	2 0	56,5	52,0	29,80		NW	1	Fair.
14	7 0	48,5	51,0	29,81		SSW	1	Cloudy.
	2 0	57,0	54,5	29,76		WSW	1	Fair.
15	7 0	48,5	52,0	29,68	0,010	SW	1	Cloudy.
	2 0	56,0	53.5	29,68		SW	1	Fair.
16	7 0	48,5	57,5	29,51		E	1	Fair.
	2 0	62,0	55,0	29,46		S	2	Rain.

METEOROLOGICAL JOURNAL

for April 1776.

Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
H. M.				Inches.	Inch.	Points.	Str.	
Apr. 17	7 0	52,0	55,0	29,58	0,132	S	2	Rain.
	2 0	65,5	60,0	29,64		S	1	Fine.
18	7 0	56,5	58,5	29,64	0,061	S	0	Cloudy.
						SW	1	Fair.
19	7 0	44,5	51,0	29,97		SW	1	Fair.
	2 0	60,5	59,0	30,00		SSW	1	Fair.
20	7 0	48,5	53,0	30,04		SW	1	Fair.
	2 0	59,0	57,5	30,07		W	1	Fair.
21	7 0	47,0	50,5	30,27		N	1	Fine.
	2 0	60,0	57,5	30,30		N by W	1	Fine.
22	7 0	48,5	54,0	30,30		S	1	Fair.
	2 0	63,0	58,5	30,25		N	1	Fair.
23	7 0	48,0	53,5	30,16		NE	1	Fair.
	2 0	62,5	59,5	30,09		NE	1	Fine.
24	7 0	48,0	55,0	29,95		NE	1	Fine.
	2 0	64,5	58,5	29,88		ENE	1	Fine.
25	7 0	50,0	57,0	29,86		SE	1	Fair.
	2 0	68,5	62,0	29,88		NW	1	Fine.
26	7 0	43,0	49,5	30,16		N	1	Fine.
	2 0	58,0	55,0	30,19		N	1	Fair.
27	7 0	47,5	51,0	30,30		NW	1	Fine.
	2 0	59,0	55,5	30,30		NE	1	Fine.
28	7 0	46,5	51,5	30,30		SW	1	Fine.
	2 0	63,5	57,5	30,20		WSW	1	Fine.
29	7 0	45,5	52,0	30,10		N	1	Fair.
	2 0	56,5	55,5	30,10		NNW	1	Fair.
30	7 0	41,0	46,0	30,18		NNW	1	Fair.
	2 30	50,5	51,0	30,19		NW	1	Cloudy.

METEOROLOGICAL JOURNAL
for May 1776.

Time. H.M.		Therm. without	Therm. within	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7 0	40,0	47,0	30,21		N	1	Cloudy.
	2 0	50,5	49,5	30,21		NNE	1	Cloudy.
2	7 0	49,0	50,0	30,18		W	1	Cloudy.
	3 0	64,5	55,5	30,11		SW	1	Fine.
3	7 0	52,0	55,0	29,70		SW	2	Cloudy.
	2 0	60,5	57,5	29,64		W by S	2	Fine.
4	7 0	46,0	50,0	29,77	0,094	NNW	2	Fine.
	2 0	61,0	55,5	29,65		WSW	1	Fair.
5	7 0	44,5	52,5	29,21	0,105	SW	1	Cloudy.
	2 0	50,5	54,0	29,14		WSW	2	Fine.
6	7 0	42,0	48,5	29,48	0,242	NWbyN	2	Fine.
	2 0	51,5	51,5	29,50		NW	2	Fair.
7	7 0	42,5	48,0	29,76		W by N	1	Fair.
	2 0	50,5	50,5	29,70		N by W	1	Fair.
8	7 0	41,5	48,0	29,66	0,167	N	1	Fair.
	2 0	52,0	51,0	29.67		NNE	2	Fair, Thunder at 1.
9	7 0	45,0	49,0	29,71	0,077	NE	1	Cloudy.
	2 0	52,0	51,5	29,78		NNE	1	Cloudy.
10	7 0	46,0	50,0	30,00	0,010	SE	1	Fine.
	2 0	58,0	53,0	30,05		ENE	1	Fine.
11	7 0	46,5	52,0	30,18	0,071	N	1	Fine.
	2 0	59,0	55,5	30,22		NNE	1	Fair.
12	7 0	53,0	54,5	30,28		SSW	1	Fair.
	2 0	62,0	58,0	30,29		SW	1	Cloudy.
13	7 0	49,0	55,5	30,45	0,067	NE	1	Fine.
	2 0	62,5	57,5	30,46		N by W	1	Fine.
14	7 0	55,0	57,5	30,43		WSW	1	Fair.
	2 0	63,5	61,5	30,36		N by W	1	Fine.
15	7 0	48,0	54,5	30,18		N	1	Rain.
	2 0	55,0	57,0	30,16		N	2	Fair.
16	7 0	46,0	58,0	30,13	0,010	N	1	Fair.
	2 0	54,0	54,5	30,02		N	1	Fine.

METEOROLOGICAL JOURNAL

for May 1776.

	Time. H. M	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 17	7 0	47,0	52,0	30,03	0,051	N	1	Fair.
	2 0	55,0	55,0	30,06		N by E	1	Fair.
18	7 0	46,0	51,5	30,06		NE by N	1	Rain.
	2 0	56,0	53,0	29,98		NE	2	Fair.
19	7 0	52,0	52,5	29,91	0,110	E by N	2	Fair.
	2 0	50,0	53,5	29,93		NE	2	Rain.
20	7 0	49,5	53,5	30,10	0,590	NNE	1	Rain.
	2 0	64,0	57,0	30,15		NE	1	Fair.
21	7 0	47,5	53,5	30,255		NE	1	Cloudy.
	2 15	58,5	56,0	30,22		NNE	1	Fine.
22	7 0	47,0	53,0	30,15		N	1	Fair.
	2 0	61,5	55,5	30,07		NE	1	Fine.
23	7 0	47,0	52,5	30,08		N	1	Fair.
	2 0	54,0	53,5	30,07		N	1	Fair.
24	7 0	46,5	48,0	30,15		NE	2	Fine.
	2 0	58,5	53,0	30,20		NNW	2	Fine.
25	7 0	44,0	49,5	30,22		S	1	Fine.
	2 0	59,0	53,5	30,16		NNW	1	Fine.
26	7 0	53,0	54,0	30,12		W	1	Fine.
	2 0	65,0	58,5	30,11		WNW	1	Fair.
27	7 0	54,0	57,5	30,09		NW	1	Cloudy.
	2 0	64,5	61,0	30,06		SSE	1	Fair.
28	7 0	57,5	59,0	30,04		SE	1	Fine.
	2 0	72,0	64,0	30,02		SW	1	Fine.
29	7 0	61,5	61,5	29,98		SSE	1	Fine.
	2 0	77,5	67,0	29,97		S	1	Fine.
30	7 0	62,0	66,0	29,87		S	1	Fair.
	2 0	66,0	68,0	29,85		S by E	2	Fair.
31	7 0	60,0	65,0	29,86		S	1	Fair.
	2 0	70,5	68,5	29,87		SSW	1	Fair.

METEOROLOGICAL JOURNAL

for June 1776.

Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
H.	M.			Inches	Inch.	Points.	Str.	
June 1	7 0	58,0	64,5	29,81	0,010	NE	1	Cloudy.
	2 0	60,0	65,5	29,75		E	1	Rainy.
2	7 0	57,5	64,0	29,90	0,215	W	1	Fair.
	2 0	73,0	67,0	29,92		E	1	Fair.
3	7 0	60,0	65.5	29,92	0,070	NNE	1	Cloudy.
	2 0	78,5	70,5	29,92		E	1	Fine.
4	7 0	66,0	70,5	29,83		NNE	1	Fine.
	2 0	74,0	72,5	29,87		SWbyW	1	Fine.
5	7 0	61,0	65,0	29,82		SW	1	Fine.
	2 0	63,5	67,5	29,75		SW	1	Cloudy.
6	7 0	56,0	63,0	29,51	0,189	W	1	Coudy.
	2 0	59,5	65,0	29,48		NW	1	Fair.
7	7 0	52,5	61,0	29,59	0,138	SW	1	Fine.
	2 0	58,0	61,5	29,63		S by W	1	Rain.
8	7 0	55,0	58,0	29,66	0,249	SW	1	Fine.
	2 0	61,5	60,5	29,68		SW	1	Fine.
9	7 0	55,0	58,5	29,85	0,041	S	1	Rain.
	2 0	63,0	61,5	29,87		S	2	Rain.
10	7 0	59,0	61,0	30,00	0,078	SSW	1	Fair.
	2 0	66,5	63,0	30,00		SSW	1	Fair.
11	7 0	58,0	61,5	29,67	0,307	SE	1	Rain.
	2 0	67,0	65,0	29,64		SSW	1	Fair.
12	7 0	57,0	62,0	29,56	0,016	E	1	Rain.
	2 0	65,5	64,0	29,57		SSE	1	Fair.
13	7 0	57,0	59,0	29,61		W	1	Fine.
	2 0	67,5	63,5	29,64		W	1	Fine.
14	7 0	59,5	63,5	29,81		SW	1	Fine.
	2 0	71,0	65,5	29,87		SW	1	Fair.
15	7 0	62,0	65,0	29,80		S	1	Fine.
	2 0	67,0	67,5	29,89		SW	1	Cloudy.
16	7 0	59,0	65,5	29,73		S	1	Rain.
	2 0	62,0	66,0	29,72		SSW	1	Cloudy.

METE-

METEOROLOGICAL JOURNAL
for June 1776.

	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	57,0	64,0	29,82	0,230	W	1	Fine.
	2	0	69,5	66,5	29,99		W	1	Fine.
18	7	0	58,0	64,5	30,14		SW	1	Fine.
	2	0	67,5	70,0	30,21		NW	1	Cloudy.
19	7	0	61,5	66,0	30,32		N	1	Fine.
	2	0	72,0	69,5	30,34		E	1	Fine.
20	7	0	60,0	63,5	30,27		SSW	1	Fine.
	2	0	74,0	68,5	30,19		S	1	Fine.
21	7	0	64,5	65,5	29,94		S	1	Cloudy.
	2	0	72,5	69,5	29,88		SW	1	Cloudy.
22	7	0	60,5	66,5	29,89		WSW	1	Fine.
	2	0	68,5	67,5	29,90		W by S	2	Fair.
23	7	0	58,0	64,5	29,95		W by S	2	Fair.
	2	0	69,5	66,5	29,93		SW	2	Fair.
24	7	0	58,0	54,5	29,76		W	1	Fine.
	2	0	67,0	65,5	29,87		SW	2	Fair.
25	7	0	55,0	62,5	30,03		W	1	Fine.
	2	0	66,0	64,0	30,02		W	1	Fine.
26	7	0	55,0	60,0	29,94		W	1	Fine.
	2	0	68,5	65,0	29,86		NE	1	Cloudy.
27	7	0	56,0	62,0	29,77		NE	1	Cloudy.
	2	0	56,5	63,5	29,79		SE	2	Rainy.
28	7	0	53,5	58,5	29,87	0,465	NNE	1	Cloudy.
	2	0	63,0	67,0	29,94		NE	1	Fair.
29	7	0	53,5	59,0	30,03	0,020	N	0	Cloudy.
	2	0	63,0	63,0	30,05		NW	1	Fair.
30	7	0	57,0	62,0	30,01		N	1	Fair.
	2	0	71,0	65,5	29,99		SWbyW	1	Fair.

METEOROLOGICAL JOURNAL

for July 1776.

Time.		Therm without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
H.M.				Inches	Inch.	Points.	Str.	
Ju y 1	7 0	55,0	62,0	29,95		W by S	1	Fair.
	2 0	69,0	67,0	30,00		W by S	1	Fair.
2	7 0	60,5	65,0	29,98		SW	1	Fair.
	2 0	69,5	68,5	29,98		SW	1	Rain.
3	7 0	5?,0	60,0	29 99	0,101	SW	1	Fair.
	2 0	71,5	70,0	29,97		WSW	1	Fair.
4	7 0	64,0	67,5	29.95		SW	1	Fair.
	2 0	76,0	72.0	-9.91		SW	1	Fair.
5	7 0	70,0	70,0	29,77		S	1	Fine.
	2 0	67,0	71,5	29,74		S by W	1	Rain.
6	7 0	63,0	67,0	29,55	0,419	S	1	Cloudy.
	2 0	65,0	69,5	29,59		SW	1	Rain.
7	7 0	59,5	69.5	29,555	0,039	SW	1	air.
	2 0	66,5	68,5	29,62		SSW	2	Cloudy.
8	7 0	58,5	64,5	29,94	0,054	SW	1	Fine.
	2 0	70,0	66,5	29 99		SW	1	Fair.
9	7 0	56,0	64,0	29,995		SSW	1	Fair.
	2 0	69,0	66,0	29,96		S by W	1	Fair.
10	7 0	58,5	6?,5	29,91		SW by W	1	Fair.
	2 0	74,0	69.0	29,94		SSW	1	Fair.
11	7 0	56,5	65,5	29,84	0,360	ENE	1	Rain.
	2 0	63,0	68,0	29,85		NE by N	1	Fair.
12	7 0	62,0	66,5	-9.855	0,150	SSW	1	Fair.
	2 0	70,5	69,5	29,86		SW	1	Fine.
13	7 0	61,0	67,0	29,77	0,198	W by N	1	Rainy.
	2 0	70,0	70,0	29.76		WSW	1	Fair.
14	7 0	62,0	67,0	29,85		SW	1	Fine.
	2 0	71,5	69,5	29,80		S by W	1	Fine.
15	7 0	62,5	67,5	29,755	0,092	S	1	ine.
	2 0	65,5	68,5	29,71		S by W	2	Fair.
16	7 0	63,0	67,0	29,58	0,064	S	1	loudy.
	2 0	73,0	70,5	29,61		SW	2	ine.

METE-

METEOROLOGICAL JOURNAL
for July 1776.

	Time. H. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 17	7 0	64,0	69,0	29,66		SSW	1	Fair.
	2 0	71,0	70,5	29,71		W by S	1	Fair.
18	7 30	62,0	67,5	29,82		SWbyW	1	Fine.
	2 0	74,0	70,0	29,82		SW	2	Fine.
19	7 0	60,5	65,5	29,74		ENE	1	Fair.
	2 0	76,0	71,5	29,68		S	1	Fair.
20	7 0	63,0	68,0	29,71		W by N	1	Fair.
	2 0	72,5	71,0	29,81		W by N	1	Fair.
21	7 0	63,0	67,0	29,87		SSW	1	Cloudy.
	2 0	69,0	69,0	29,85		SSW	1	Fair.
22	7 0	61,0	65,0	29,80	0,275	E by S	0	Cloudy.
	2 0	66,0	67,0	29,84		NE	1	Cloudy.
23	7 0	59,0	65,0	29,90		SW	1	Fine.
	2 0	74,0	79,5	29,95		SWbyW	1	Fine.
24	7 0	63,0	68,5	30,03		SW	1	Fair.
	2 0	67,0	69,5	30,05		S	1	Rainy.
25	7 0	57,0	63,5	30,22		SSW	1	Fine.
	2 0	69,0	67,5	30,23		SW	1	Fine.
26	7 0	61,0	67,0	30,13		SW	1	Fine.
	2 0	82,0	73,0	30,08		S by W	1	Fair.
27	7 0	62,5	67,5	30,15		SW	1	Fine.
	2 0	73,0	70,5	30,18		W	1	Fair.
28	7 0	59,5	67,0	30,22		SW	1	Fine.
	2 0	71,5	70,5	30,17		SSW	1	Fair.
29	7 0	62,0	66,0	30,18		NW	1	Fine.
	2 0	72,0	70,5	30,22		W by N	1	Fine.
30	7 0	61,0	65,0	30,31		NE	1	Fair.
	2 0	74,0	70,5	30,29		E by N	1	Fine.
31	7 0	62,0	65,0	30,24		SE	1	Fine.
	2 0	75,0	72,0	30,21		E by S	1	Fine.

METE-

METEOROLOGICAL JOURNAL
for August 1776.

	Time.	Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7 0	65,0	66,0	30,16		SW	1	Fair.
	2 0	74,0	67,0	30,14		S by W	1	Fine.
2	7 0	67,0	68,5	30,13		E	1	Fair.
	2 0	86,0	77,0	30,09		S	1	Fine.
3	7 0	62,0	72,5	30,09	0,052	W by S	1	Fine.
	2 0	76,0	75,0	30,06		N	1	Fine.
4	7 0	63,5	68,5	30,03		W by N	1	Fair.
	2 0	73,0	73,0	30,00		NNW	1	Fair.
5	7 0	66,0	70,5	29,92		SW	1	Fine.
	2 0	79,5	74,5	29,85		WSW	1	Fine.
6	7 0	63,5	71,5	29,77		NW	1	Fair.
	2 0	72,0	72,5	29,73		W by N	1	Fair.
7	7 0	56,0	64,0	29,82		SW	1	Fine.
	2 0	67,5	67,0	29,84		W	1	Fair.
8	7 0	55,0	61,0	29,64	0,262	SE	1	Rainy.
	2 0	65,0	64,5	29,76		NW	1	Fine.
9	7 0	59,5	63,5	29,80	0,067	SE by S	1	Fine.
	2 0	66,5	66,0	29,71		SSE	2	Cloudy.
10	7 0	57,0	63,5	29,53	0,452	SSW	1	Rainy.
	2 0	66,0	65,5	29,65		W	2	Fair.
11	7 0	67,0	65,0	29,76	0,129	SE	1	Fair.
	2 0	66,5	66,5	29,71		S	1	Rain.
12	7 0	60,0	64,0	29,75	0,306	SW	1	Fine.
	2 0	66,5	66,0	29,73		SSW	1	Rain.
13	7 0	59,0	64,0	29,70	0,169	SW by S	1	Fine.
	2 0	67,5	68,0	29,70		W	2	Fine.
14	7 0	55,5	63,5	29,96	0,054	SW	1	Fine.
	2 0	71,5	68,0	29,98		SW	1	Fine.
15	7 0	64,5	67,0	29,88	0,022	SSW	1	Cloudy.
	2 0	73,5	71,5	29,80		SW by S	1	Fine.
16	7 0	63,0	67,5	29,61	0,161	S by W	1	Rain.
	2 0	69,5	70,0	29,63		SSW	2	Fair.

METEOROLOGICAL JOURNAL

for Auguſt 1776.

Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
H. M.				Inches.	Inch.	Points.	Str.	
Aug. 17	7 0	55,0	63,0	29,76	0,201	WSW	1	Fine.
	2 0	67,0	66,0	29,81		WSW	2	Fair.
18	7 0	54,0	62,0	29,875	0,018	W	1	Fine.
	2 0	69,5	65,5	29,88		W	1	Fine.
19	7 0	61,5	64,0	29,64	0,139	S	1	Rain.
	2 0	69,5	68,0	29,64		SW	1	Fair.
20	7 0	59,0	65,5	29,69	0,250	W by S	2	Fair.
	2 0	67,5	67,0	29,82		W by S	2	Fine.
21	7 0	57,5	63,0	30,08		ENE	1	Fair.
	2 0	67,0	67,0	30,09		NE	1	Fine.
22	7 0	54,5	58,0	30,07		NNW	1	Fine.
	2 0	67,0	65,5	30,06		NW	1	Fine.
23	7 0	57,0	62,5	30,06		NE	1	Fair.
	2 0	70,0	66,5	30,05		NE	1	Fine.
24	7 0	54,0	62,0	30,06		W by N	1	Fine.
	2 0	64,5	64,5	30,07		NW	1	Fair.
25	7 0	53,0	61,0	30,10		W	1	Fine.
	2 0	66,0	64 0	30,13		NW	1	Fine.
26	7 0	57,5	61,5	30,07		E	1	Cloudy.
	2 0	67,0	66,0	30,03		S	1	Cloudy.
27	7 0	57,0	62,5	29,93	0,050	E	1	Cloudy.
	2 0	64,5	66,5	29,93		E	1	Fair.
28	7 0	53,0	60,0	29,85		NE	1	Fine.
	2 0	69,5	65,5	29,84		SE	1	Fair.
29	7 0	59,0	62,0	29,55	0,050	S by E	1	Rainy.
	2 0	69,0	67,5	29,99		SW	1	Fair.
30	7 0	57,0	64,5	29,60	0,136	S	1	Fine.
	2 0	66,0	67,5	29,60		SW	1	Fair.
31	7 0	54,0	63,5	29,69	0,022	SSW	1	Fine.
	2 0	70,0	68,5	29,66		SSW	1	Fine.

METEOROLOGICAL JOURNAL

for September 1776.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7 0	55,5	54,0	29,48	0,120	SW	1	Fine.
	2 0	68,5	68,5	29,52		SSW	2	Fine.
2	7 0	58,5	64,5	29,30	0,248	S	2	Fine.
	2 0	63,5	67,5	29,26		SW	2	Fine.
3	7 0	56,0	62,5	29,29	0,358	SW	1	Rainy.
	2 0	63,0	60,0	29,41		SSW	2	Cloudy.
4	7 0	55,0	62,5	29,41	0,084	SW	1	Fair.
	2 0	63,0	65,5	29,49		W by N	1	Fine.
5	7 0	51,0	61,0	29,65	0,054	SW	1	Fair.
	2 0	63,0	65,0	29,72		NW	1	Fair.
6	7 0	53,0	60,5	29,92	0,045	SSW	1	Fair.
	2 0	65,5	65,0	30,00		W by S	1	Fair.
7	7 0	52,5	60,0	29,84		S	1	Fine.
	2 0	66,5	65,5	29,75		S by E	1	Fair.
8	7 0	50,0	61,0	29,48	0,160	SW	1	Fair.
	2 0	57,5	63,5	29,48		NW	1	Fine.
9	7 0	48,0	57,0	29,90	0,058	N by W	1	Fine.
	2 0	66,5	63,5	29,97		NE	1	Fine.
10	7 0	53,0	60,0	30,11		N	1	Cloudy.
	2 0	63,0	63,5	30,12		SSW	1	Fair.
11	7 0	57,0	61,5	30,10		SW	1	Fine.
	2 0	68,5	66,5	30,11		SW	1	Fair.
12	7 0	60,0	64,0	30,07		SSW	1	Cloudy.
	2 0	68,5	68,0	30,08		S	1	Fair.
13	7 0	57,0	64,5	30,07		S	1	Fair.
	2 0	67,0	67,0	30,00		S by W	1	Fine.
14	7 0	57,0	64,0	29,90		NW	1	Fair.
	2 0	64,0	68,0	29,92		NW	1	Fair.
15	7 0	51,5	62,5	29,99		NE	1	Cloudy.
	2 0	61,0	65,0	29,88		NE	1	Cloudy.
16	7 0	61,5	53,0	29,91	0,377	NW	1	Cloudy.
	2 0	61,5	64,5	29,98		W by N	1	Fine.

METEOROLOGICAL JOURNAL
for September 1776.

	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H. M.				Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	55,0	62,0	29,99		WSW	1	Cloudy.
	2	0	54,5	63,0	29,84		E by N	1	Rain.
18	7	0	49,0	57,5	29,86	0,638	N	1	Cloudy.
	2	0	57,0	61,0	29,94		N	2	Fair.
19	7	0	47,0	54,0	30,05	0,082	N	1	Fair.
	2	0	50,0	57,5	30,12		NE	1	Rain.
20	7	0	44,5	52,5	30,25	0,178	NNE	1	Fine.
	2	0	52,0	57,5	30,26		NNE	1	Fine.
21	7	0	43,0	52,0	30,30	0,160	N	1	Fine.
	2	0	57,5	57,0	30,35		E	1	Fair.
22	7	0	46,5	52,5	30,33		NNE	1	Fair.
	2	0	59,0	57,5	30,23		E	1	Fine.
23	7	0	45,5	53,5	29,99		NE	1	Fine.
	2	0	60,0	58,0	29,92		NE	1	Fair.
24	7	0	48,0	55,0	29,91		N	1	Fair.
	2	0	63,5	60,5	29,71		NE	1	Fine.
25	7	0	54,0	57,5	29,70		SE	1	Cloudy.
	2	0	66,0	62,5	29,57		SE	1	Fine.
26	7	0	56,5	61,5	29,30	0,131	S	1	Fine.
	2	0	63,0	65,0	29,30		S	2	Fair.
27	7	0	48,0	58,5	29,55		S	1	Fair.
	2	0	61,0	62,5	29,64		S by W	1	Fine.
28	7	0	57,0	60,0	29,52	0,019	SSW	1	Rain.
	2	0	62,0	63,0	29,59		SW	1	Fine.
29	7	0	49,5	59,0	29,80		WNW	1	Fine.
	2	0	61,5	62,5	29,97		NE	1	Fine.
30	7	0	51,0	58,0	29,99		NNE	1	Fair.
	2	0	61,5	62,5	30,00		NE	1	Fine.

METEOROLOGICAL JOURNAL

for October 1776.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7 0	52,0	58,5	29,92		NE	1	Fair.
	2 0	59,5	62,0	29,92		ENE	1	Fair.
2	7 0	54,5	60,0	30,05		NE	1	Cloudy.
	2 0	66,5	64,5	30,10		NNE	1	Fair.
3	7 0	55,0	62,0	30,21		NE	1	Cloudy.
	2 0	60,0	64,0	30,21		N	1	Cloudy.
4	7 0	53,0	65,0	30,10		SSE	1	Cloudy.
	2 0	58,0	63,0	29,98		NW	1	Fair.
5	7 0	46,5	57,5	29,83		WNW	1	Fair.
	2 0	56,5	60,5	29,80		NNW	1	Fair.
6	7 0	44,5	55,0	29,78		NNE	1	Fair.
	2 0	55,0	57,5	29,77		NNW	1	Fine.
7	7 0	42,0	52,0	29,76		SSW	1	Fine.
	2 0	58,5	57,5	29,65		SW	2	Rain.
8	7 0	49,0	55,0	29,84	0,020	S	1	Fair.
	2 0	61,0	59,5	29,75		S	2	Cloudy.
9	7 0	43,5	54,5	30,17	0,012	S	1	Fine.
	2 0	58,0	58,5	30,23		WSW	2	Fine.
10	7 0	57,0	58,5	30,12		WSW	1	Fair.
	2 0	66,5	63,5	30,18		W	1	Fair.
11	7 0	53,0	60,0	30,17		WSW	1	Fine.
	2 0	62,5	64,5	30,19		NNW	1	Fair.
12	7 0	49,5	58,5	30,28		N	1	Fair.
	2 0	57,5	61,5	30,32		NE	1	Fine.
13	7 0	52,0	58,0	30,28		SW	1	Cloudy.
	2 0	58,5	62,0	30,24		NW	1	Cloudy.
14	7 0	51,0	59,0	30,17		W	1	Cloudy.
	2 0	57,0	61,5	30,16		S	1	Cloudy.
15	7 0	57,5	58,5	30,16		NNW	1	Cloudy.
	2 0	57,0	61,5	30,10		SE	1	Cloudy.
16	7 0	44,0	53,5	29,92		NE	1	Foggy.
	2 0	61,0	60,0	29,88		S by E	1	Fair.

METE-

METEOROLOGICAL JOURNAL
for October 1776.

	Time.		Therm. without.	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
	H	M							
Oct. 17	7	0	58,0	60,0	29,80		S	1	Fair.
	2	0	56,0	62,5	29,85		NNW	1	Rain.
18	7	0	43.5	55,0	30,23	0,152	SW	1	Fair.
	2	0	59,0	60,0	30.19		S	1	Fine.
19	7	0	52,5	57,0	29,86		E by N	1	Foggy.
	2	0	61,5	61,5	29,80		S	1	Rain.
20	7	0	56,0	59,0	29,73		S	1	Fair.
	2	0	56,0	62,0	29,84		W by N	1	Rain.
21	7	0	46,0	55,5	30,01	0,356	SSE	1	Cloudy.
	2	0	59,0	60,0	29,97		S by E	1	Fine.
22	7	0	55,5	58,0	29,99		S	1	Fair.
	2	0	65,0	64,0	30,04		S	1	Rain.
23	7	0	55,0	60,0	30,10		NE	1	Cloudy.
	2	0	56,0	62,0	30,12		NE	1	Cloudy.
24	7	0	56,0	60,0	30,14		E by N	1	Cloudy.
	2	0	55,0	61,5	30,14		NE	1	Cloudy.
25	7	0	51,0	56,5	30,01		NE	1	Cloudy.
	2	0	54,0	59 0	29,93		NE by E	1	Fair.
26	7	0	48,0	54,0	29,84		N	1	Cloudy.
	2	0	51,0	56,5	29,85		NNE	1	Fair.
27	7	0	43,5	51,5	29,90		NNW	1	Cloudy.
	2	0	52,0	55,5	29,93		N	1	Fine.
28	7	0	38,0	49,5	30,04		NE	1	Fair.
	2	0	51,0	55,0	30,04		E	1	Fine.
29	7	0	49,0	52,5	30,03		SE	1	Cloudy.
	2	0	53,0	55,5	30,00		ESE	1	Rain.
30	7	0	49,0	54,0	30,01	0,194	SE	1	Fair.
	2	0	54,0	57,5	30,96		SSE	1	Fine.
31	7	0	43,0	51,0	29,85		ENE	1	Cloudy.
	2	0	48,0	53,5	29,89		NE	1	Fine.

METEOROLOGICAL JOURNAL
for November 1776.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	37,5	47,5	29,94		E	1	Fair.
	2	0	50,0	52,0	29,82		S by E	1	Fair.
2	7	0	39,0	48,5	29,78	0,165	SSW	1	Fair.
	2	0	49,5	52,0	29,78		W by N	1	Fair.
3	7	0	33,5	45,0	30,06		S	1	Fair.
	2	0	44,0	49,0	30,11		SW	1	Fair.
4	7	0	38,5	44,5	30,16		SW	1	Fine.
	2	0	54,0	50,0	30,14		S	1	Fine.
5	8	0	49,0	50,0	30,18		SSW	1	Fair.
	2	0	54,0	54,0	30,19		NW	1	Cloudy.
6	8	0	36,0	47,5	30,17		NE	1	Foggy.
	2	0	44,0	50,5	30,10		NE	1	Fair.
7	8	0	40,0	46,5	30,02		NE	1	Foggy.
	2	0	51,5	51,0	30,00		NE	1	Foggy.
8	8	0	40,0	49,0	30,01		NE	1	Foggy.
	2	0	54,0	56,0	29,98		NE	1	Foggy.
9	8	0	40,5	47,0	29,92		N by E	1	Foggy.
	2	0	55,0	51,5	29,92		S by E	1	Fair.
10	8	0	49,0	51,5	29,97		SE	1	Foggy.
	2	30	52,5	54,5	29,97		S	1	Cloudy.
11	8	0	49,0	53,5	29,90		S	1	Fair.
	2	0	50,5	56,0	29,85		SE	1	Cloudy.
12	8	0	42,0	52,5	30,02	0,136	SW	1	Fair.
	2	0	52,0	55,5	30,11		W by S	1	Fine.
13	8	0	48,0	53,0	30,13		SW	1	Fine.
	2	0	55,5	56,5	30,13		SW	1	Fine.
14	8	0	52,5	55,5	30,23	0,010	SW	1	Cloudy.
	2	0	57,0	58,5	30,21		SW	1	Cloudy.
15	8	0	48,5	55,5	30,22	0,025	SW	1	Fair.
	2	0	56,0	57,5	30,12		SW	1	Fair.
16	8	0	57,5	58,0	29,73		SW	1	Cloudy.
	2	0	55,0	59,5	29,95		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for November 1776.

Time. H. M.		Therm. without	Therm. within	Barom. Inches	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Nov. 17	8 0	45,5	55,0	29 67	0,010	SW	1	Fair.
	2 0	49,5	56,0	29.77		W	1	Fair.
18	8 0	42,0	50,0	29,87		W	1	Fair.
	2 0	51,5	53,0	29,74		SW	1	Fair.
19	8 0	49,0	53,0	29,71		S	0	Rain.
	2 0	52,0	55,0	29,67	0,140	SE	1	Rain.
20	8 0	53,0	56,0	29,32		SW	2	Fair.
	2 0	58,0	59,5	29,36	0,089	SW	2	Rain.
21	8 0	40,0	49,5	29,70		WNW	2	Fine.
	2 0	45,0	51,0	29,75	0,080	SW	1	Fine.
22	8 0	33,0	45,0	29,94		SW	1	Fair.
	2 0	43,0	48,0	29,89		W	1	Fair.
23	8 0	40,0	45,5	29,36		S by E	1	Rain.
	2 0	43,5	48,0	29,23	0,840	SE	1	Rain.
24	8 0	36,0	44,0	29,61		N by E	2	Fair.
	2 0	42,0	46,5	29,94	0,176	NW	2	Fine.
25	8 0	33,0	41,0	30,16		N	1	Fine.
	2 0	41,0	45,0	30,20		N by E	1	Fine.
26	8 0	28,0	39,5	30,39		E by N	1	Foggy.
	2 0	37,0	43,5	30,31		NE	1	Fine.
27	8 0	31,0	37,0	30,09		E	1	Frosty.
	2 0	36,0	40,0	29,97		SE	1	Fair.
28	8 0	34,0	48,5	29,655		SE	1	Cloudy.
	2 0	35,0	42,0	29,52		SE	1	Cloudy.
29	8 0	38,0	40,5	29,11		SE	1	Rain.
	2 0	46,5	45,0	29,11		SE	1	Cloudy.
30	8 0	41,0	45,5	29,37	0,520	SW	1	Rain.
	2 0	47,0	40,0	29,44		SW	1	Fine.

METE-

METEOROLOGICAL JOURNAL
for December 1776.

Time.		Therm without	Therm. within	Barom.	Rain.	Winds.		Weather.
H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1 8	0	50,0	50,0	29,48	0,222	S	2	Fair.
2	0	54,0	54,0	29,69		SSW	2	Cloudy.
2 8	0	47,0	51,5	30,04	.	SE	1	Rain.
2	0	52,5	54,5	29,97		E	1	Fair.
3 8	0	44,5	51,5	29,985		E	1	Foggy.
2	0	51,0	54,0	29,96		SE	1	Fair.
4 8	0	50 5	53,5	29,95		SSW	1	Foggy.
2	0	55,0	56,0	29,97		S by W	1	Cloudy.
5 8	0	49,5	50,0	30,01		SE	1	Cloudy.
2	0	53,0	56,0	30,06		N by E	1	Cloudy.
6 8	0	47,0	54,5	29,95	0,052	NE	1	Cloudy.
2	0	48,0	55,0	29,91		NE	1	Cloudy.
7 8	0	42,0	50,0	29,95		ENE	1	Fair.
2	0	43,5	52,0	29,97		S by E	1	Fine.
8 8	0	43,0	47,0	30,05		E	1	Fair.
2	0	50,5	52,0	30,06		SE	1	Fair.
9 8	0	42,0	49,5	30.10		SSE	1	Fine.
2	0	44,0	49,5	30,11		SE	1	Fine.
10 8	0	42,0	47,0	30,20		S	1	Cloudy.
2	0	46,0	48,5	30,25		SW	1	Cloudy.
11 8	0	46,0	48,5	30,35		SW	1	Fine.
2	0	53,0	51,5	30,41		SW	1	Fine.
12 8	0	49,0	52,0	30,42		SW	1	Cloudy.
2	0	57,5	52,5	30,39		W	1	Cloudy.
13 8	0	47,0	57,5	30,34	0,057	NE	1	Cloudy.
2	0	47,0	52,0	30,32		NE	1	Rain.
14 8	0	46,0	50,5	30,33	0,055	SSW	1	Cloudy.
2	0	47,0	51,0	30,27		SE	1	Cloudy.
15 8	0	39,0	47,0	30,09		SE	1	Fair.
2	0	42,0	47,0	29,99		SW	1	Cloudy.
16 8	0	40,0	46,0	29,73	0,409	N	1	Rain.
2	0	44,0	46,0	29,73		NW	1	Fine.

METE-

METEOROLOGICAL JOURNAL
for December 1776.

Time. H. M.		Threm. without.	Therm: within.	Barom. Inches	Rain. Inch	Winds Points.	Str	Weather.
Dec. 17	8 0	30,0	41,0	29,755	0,020	NNW	1	Fine.
	2 0	36,0	41,0	29,755		NW	1	Fine.
18	8 0	26,0	35,0	29,59		WNW	1	Cloudy.
	2 0	32,0	35,0	29,55		S by W	1	Frosty.
19	8 0	38,0	36,5	29,16	0,139	W by N	2	Rain.
	2 0	40,0	3 ,0	29,32		W	1	Fair.
20	8 0	35,0	37,0	29,47		SSW	1	Cloudy.
	2 0	36,0	39,0	29,47		NE	1	Rain.
21	8 0	52,0	39,0	29,69	0,109	SW	1	Rain.
	2 0	54,0	41,0	29,58		SW	1	Rain.
22	8 0	52,0	46,0	29,765	0,053	WSW	2	Cloudy.
	2 0	54,0	40,0	29,84		WSW	1	Fair.
23	8 0	42,0	48,0	29,87	0,050	SW	1	Fair.
	2 0	46,5	48,5	29,81		WNW	1	Fine.
24	8 0	39,0	44,0	29,57	0,022	NW	2	Fair.
	2 0	39,0	44,0	29.51		W by N	2	Rain.
25	8 0	35,5	38,5	29,77	0,047	NW	2	Fine.
	2 0	39,0	40.0	29,82		N by W	1	Fine.
26	8 0	30,0	35,5	29,81		N	1	Fine.
	2 0	36,0	37,0	29,76		NE	1	Rain.
27	8 0	33,0	36,0	29.98		N	1	Cloudy.
	2 0	36,0	37,0	29 87		NE	1	Cloudy.
28	8 0	29,0	35,0	30,00		N	1	Fair.
	2 0	33,0	35,0	30,02		NNE	1	Fair.
29	8 0	32,0	35,5	29 73		N	1	Fair.
	2 0	36,5	34,5	29,64		W by N	1	Fair.
30	8 0	32,0	34 5	29,47	0,040	N	1	Snowy.
	2 0	29,0	34,0	29,49		N by E	1	Fine and frosty.
31	8 0	25,0	30,5	29,71		N	1	Fair and frosty.
	2 0	31,0	31,0	29,88		NW	1	Fair and frosty.

1776.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January	44,5	13,5	29,3	43,5	20,5	31,8	30,14	29,21	29,687	1,157
February	49,5	14,5	42,6	46,0	19,0	42,4	29,97	28,84	29,408	3,510
March	52,0	30,5	45,9	56,0	37,0	46,4	30,43	28,98	29,91	1,541
April	56,5	34,5	50,3	58,0	41,0	51,2	30,44	29,46	29,542	0,320
May	62,0	40,0	54,3	66,0	47,0	54,9	30,46	29,14	29,999	1,594
June	66,0	32,5	62,4	70,5	54,5	64,2	30,34	29,48	29,87	2,028
July	70,0	55,0	65,9	70,0	60,0	68,3	30,31	29,55	29,913	1,752
August	67,0	53,0	64,2	72,5	58,0	66,1	30,16	29,53	29,546	2,540
September	61,5	43,0	57,2	64,5	52,0	66,1	30,35	29,26	29,83	2,712
October	58,0	42,0	53,8	65,0	51,0	53,4	30,32	29,65	30,00	0,734
November	57,0	28,0	45,0	58,0	37,0	49,9	30,39	29,11	29,878	1,191
December	52,0	25,0	42,0	57,5	30,5	45,0	30,42	29,16	29,882	1,275
Whole Year,			51,1			52,9			29,789	20,354

VARIATION

VARIATION-NEEDLE.

	7 h. A.M.	12 h. M	2 h. P.M.	10 or 11 h. P.M.	Daily Mean.
June 21			21 52	21 45	21 49
22	21 38	21 49	21 52	22 20	21 55
23	21 43	21 42	21 43	21 47	21 44
24	21 41	21 49	21 54	21 36	21 45
25	21 57	21 54	21 53	21 41	21 51
26	21 43	21 47	21 49	21 33	21 43
27	21 38	21 48	21 50	21 37	21 43
28	21 43	21 47	21 40	21 37	21 42
29	21 59	21 51	21 45	21 36	21 48
30	21 39	21 49	21 44	21 34	21 42
July 1	22 00	21 51	21 50	21 40	21 50
2	21 36	21 50	21 54	21 46	21 47
3	21 59	21 49	21 50	21 46	21 51
4	21 40	21 55	21 52	21 47	21 49
5	21 40	21 56	21 56	21 46	21 49
6	21 40	21 50	21 53	21 35	21 49
7	22 00	21 49	21 49	21 53	21 53
Means,	21 46	21 50	21 50	21 43	

Mean of all 21 47.

DIPPING-

DIPPING-NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Mean.	
	° ′	° ′	° ′	° ′	° ′	
June 21			71 32	71 36		
22	72 22	71 40	71 40	71 45	71 43	West mark up-permost.
23	71 45	71 50	71 45	71 30		
24	71 50	71 35	71 45	71 35		
25	71 37	73 10	73 00	72 45		
26	73 20	72 50	73 05	73 00	73 01	East.
27	73 05	73 00	73 15	72 20		
28	73 10	71 50	71 50	71 50		
29	72 05	72 20	72 00	71 40		
30	71 35	71 50	71 50	72 10	71 56	East mark down.
July 1	72 00	71 50	71 50	71 50		
2	71 55	70 50	71 52	71 50		
3	71 50	71 50	71 55	72 00		
4	72 00	71 57	73 10	73 30		
5	73 20	73 25	73 20	73 30	73 22	West.
6	73 30	73 24	73 20	73 20		
7	73 25	73 15	73 20	73 25		
					72 30	

ERRATA in VOL. LXVI.

*** There are SIX PLATES in the FIRST PART of the PHILOSOPHICAL TRANSACTIONS, Vol. LXVII.   But the SECOND of them, containing Two diftinct Subjects, is numbered TAB. II. at the Head, and TAB. III. below.——This is mentioned, left the Binder, attending to the Number at the Head only, fhould fuppofe the THIRD Plate to be wanting.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

VOL. LXVII.   D d d

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

V O L. LXVII. For the Year 1777.

P A R T   II.

[v]

CONTENTS

TO

VOL. LXVII. PART II.

XXXI. *A*

XX. *An Account of a Volcanic Hill near* Invernefs. *In a Letter from* Thomas Weft, *Efq. to Mr.* Lane, *F. R. S.*

DEAR SIR,

Bath,
Oct. 27, 1776.

Read Dec. 12, 1776.

A S you feem defirous to have a more par- ticular account of the lava, which I left at your houfe on my return from my tour in Scotland, I herewith fend it you. At the fame time I muft own, that the little attention paid to fo extraordinary a phenomenon, and which feems to prove beyond a doubt the exiftence of volcanos in this country, is to me matter of great furprize.

The hill from whence I took the fpecimens of lava which I have the pleafure to fend you, is about a mile

and an half diftant from Invernefs, and is called by the inhabitants Creck Faterick, or Peter's Rock: the lower part of it is chiefly ploughed up; the upper part is extremely fteep, craggy, and very difficult of accefs, and appears to me to have evident marks of having been a volcano, as by much the greateft part of the largeft rocks on it have been either ftrongly calcined or fufed, as will, I think, plainly appear by thefe fpecimens, fome of which I picked up on the fides of the hill, others I broke off from the folid rock with a pick-axe (though not without great difficulty, on account of the extreme hardnefs of the rock); and others I dug out from the fummit of the hill, at the depth of four feet, lying in a rich, light, black mould, which, after having been expofed to the air for fome time, turned to a whitifh afh colour.

On the fummit of this hill (which commands a moft beautiful and extenfive profpect) is a fmall plane, ninety paces long, by twenty-feven wide, furrounded by rocks, from fix to eight feet high, like a breaft-work, extremely craggy, and difficult of accefs on the outfide, but rifing from the center, in the infide, to the top of the breaft-work, with a gentle flope of turf, equal to any of the fineft on our fheep downs. This I at firft thought might have been the crater; but the fmoothnefs of the infide feeming not to favour this opinion, I carefully examined

2

every part of the hill, in hopes of finding fome marks of one, but without fuccefs. There are, indeed, feveral fmall caves, but of no great depth, which feem to be formed by the hanging rocks on the fides, near the top of the hill. There is alfo a fmall fpring about fifty yards from the top of the hill, but it was almoft dry when I faw it, which was the latter end of July. I could not get, from the beft inquiries I was able to make, any account of this extraordinay phenomenon, no perfon in Invernefs (except one gentleman of that city) having ever taken the leaft notice of it; nor can I find that any mention has ever been made of it in hiftory, though it feems probable that there has been at leaft one more in this part of the country; as a gentleman near Dingwal, which is about twenty miles from Invernefs, affured me, that there were many ftones on a hill near his houfe which they called the vitrified fortification, that had the fame appearance with thofe at Creck Faterick; but that no perfon had ever taken any particular notice of them[a].

I am, &c.

(a) The fpecimens fent with the above letter to the Royal Society, having been examined by fome members well acquainted with volcanic produćtions, were by them judged to be real lava; and it was their opinion, that if a great quantity of the like fubftance be found on the hill from whence thefe pieces were taken, the hill moft probably owes its origin to a volcano. J. P.

XXI. *New Electrical Experiments and Observations; with an Improvement of Mr.* Canton's *Electrometer. By Mr.* Tiberius Cavallo, *in a Letter to Mr.* Henly, *F. R. S.*

DEAR SIR,

Read March 13, 1777. TOGETHER with this letter I send you an account of some electrical experiments I have lately made, and most of which have been successfully repeated in your presence. As you did me the honour to mention, in your last paper to the Royal Society, some remarks I had made on Mr. VOLTA's machine, I hope you will farther oblige me by presenting this account to that learned Body, if you think it contains any thing deserving the attention of the curious.

I am, &c.

Experiments

Experiments on Mr. VOLTA's *plates, commonly called a machine for exhibiting perpetual electricity.*

THE following experiments, which shew how both fides of an electric plate are affected in different circumstances, were principally made with a plate which meafured fix inches and a quarter in diameter, and which confifted of a circular piece of thick glafs coated on one fide with fealing-wax[a].

If, after having excited the fealing-wax, I lay the plate with the wax upon the table, and the glafs uppermoft, that is, contrary to the common method; then, on making the ufual experiment of putting the metal plate on it, and taking the fpark, &c. I obferve it to be attended with the contrary electricity; that is, if I lay the metal plate upon the electric one, and, while in that fituation, touch it with an infulated body, that body acquires the

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(a) Having conftructed feveral of thofe plates, with a view to difcover which fubftance would anfwer the beft for coating the glafs plate, I obferved that the eafieft to be made, and the ftrongeft in power, are thofe made of the fecond fort of fealing-wax. It is remarkable, that fometimes they will not act well at firft, but they may be rendered very good by fcraping with the edge of a knife their fhining or gloffy furface. This feems analogous to the well-known property of glafs, which is, that new cylinders or globes made for electrical purpofes are often very bad electrics at firft, but that they improve by being worked, that is, by having their furface a little worn.

pofitive

positive electricity, and the metallic removed from the electric plate appears to be negative.

This experiment, I find, answers in the same manner if an electric plate be used which has the sealing-wax coating on both sides; for, whichever side of this is excited, it will act like the waxed side of the above described plate, that is, will render the metal plate set on it positive, and the opposite side will render it negative.

If, instead of laying the electric plate upon the table, it be placed upon an electric stand so as to be accurately insulated, then the metal plate set on it acquires so little electricity that it can only be discovered with an electrometer; which shews, that the electricity of this plate will not be conspicuous on one side of it, if the opposite side be not at liberty either to part with, or acquire more of, the electric fluid. In consequence of this experiment, and in order to ascertain how the opposite sides of the electric plate would be affected in different circumstances, I made the following experiments.

Upon an electric stand I placed a circular tin plate, nearly six inches in diameter, which by a slender wire communicated with an electrometer of pith-balls, which was also insulated; I then placed the excited electric plate of six inches and a quarter diameter upon the tin plate, with the wax uppermost, and on removing my hand

from

from it, the electrometer which communicated with the tin plate, that is, with the under ſide of the electric plate, immediately opened with negative electricity. If by touching the electrometer I take that electricity off, the electrometer will not afterwards diverge. But if now, or when the electrometer diverges, I preſent my hand open, or any other uninſulated conductor, at about one or two inches over the electric plate, without touching it, then the pith-balls diverge; or, if they diverged before, they come together, and immediately diverge again with poſitive electricity.——Remove the hand, and the balls come together; approach the hand, and they diverge, and ſo on.

If while the pith-balls diverge with negative electricity, I put the metal plate upon the wax, the balls approach each other for a little time, but ſoon open again with the ſame, that is with negative electricity.

If, whilſt the metallic lies upon the electric plate, I touch the former, the electrometer immediately diverges with poſitive electricity, which if by touching the electrometer I take off, the electrometer continues without divergence. I touch the metal again, and the electrometer opens again; and ſo on for a conſiderable number of times, until the metal plate has acquired its full charge. On taking now the metal plate up, the electro-

meter inftantly diverges with ftrong negative elec-
tricity.

I repeated the above experiments with this only dif-
ference in the difpofition of the apparatus: I put the
electric plate with the excited fealing-wax upon the
circular tin-plate, and the glafs uppermoft: and the dif-
ference in the refult was, that where the electricity was
pofitive in the former difpofition of the apparatus, it now
became negative, and *vice verfa*; except that, when I
firft lay the electric plate upon the tin plate, the electro-
meter diverged with negative electricity as well in this
as in the other difpofition of the apparatus.

All the above experiments have been repeated with
an electric plate, which, befides the fealing-wax coating
on one fide, had a ftrong coat of varnifh on the other;
and their refult has been fimilar to thofe when the above
defcribed plate was ufed.

Experiments on colours.

Having accidentally obferved that an electric fhock,
fent over the furface of a card, marked a black ftroke
upon a red fpot on the card, I was from this induced to
try what would be the effect of fending fhocks over
cards painted with different water colours. Accordingly

4   I painted

I painted several cards with almost every colour I had, and sent shocks over them when they were very dry: the effects were as follows[b]:

Vermillion was marked with a strong black track, about one-tenth of an inch wide.

Carmine received a faint and slender impression, of a purple colour.

Verdigrease was shaken off from the surface of the card.

White lead was marked with a strong black track, not so broad as that on vermillion.

Red lead was marked with a faint mark much like carmine.

The other colours I tried were, orpiment, gamboge, sap-green, red ink, ultramarine, Prussian blue, and a few others, which were compounds of the above, but they received no impression.

It having been insinuated, that the strong black mark, which vermillion receives from the electric shock, might possibly be owing to the great quantity of sulphur contained in that mineral, I was induced to make the following experiment. I mixed together equal quantities of orpiment and flower of sulphur, and with this mix-

(b) The force employed was the full charge of one foot and a half of coated glass.

ture, by the help, as ufual, of very diluted gum-water, I painted a card; but the electric fhock fent over this left not the leaft impreffion.

Defirous of carrying this inveftigation on colours a little farther, I procured fome pieces of paper painted on both fides with oil colours[c], and fending the charge of two feet of coated glafs over each of them, I obferved that the pieces of paper painted with lamp-black, Pruffian blue, vermillion, and purple brown, were torn by the explofion; but white lead, Naples yellow, Englifh ochre, and verdigreafe, remained unhurt.

The fame fhock fent over a piece of paper, painted very thick with lamp-black and oil, left not the leaft impreffion. I fent the fhock alfo over a piece of paper, unequally painted with purple-brown, and the paper was torn where the paint laid very thin, but remained unhurt where the paint was evidently thicker.

Having repeated thofe experiments feveral times, and with fome little variation, they were attended with different effects; however, they all feem to point out the following propofition.

I. A coat of oil-paint over any fubftance, defends it from the effects of fuch an electric fhock as would otherwife injure it; but by no means defends it from the force

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[c] The colours were mixed with linfeed oil.

I

of

of every electric fhock that can be given. II. No one colour feems preferable to the others, if they be equal in fubftance, and equally well mixed with oil; but a thick coating does certainly afford a better defence than a thinner one.

By rubbing the above mentioned pieces of paper, I find that the paper painted with lamp-black and oil is more eafily excited, and acquires a ftronger electricity than the papers painted with the other colours; and perhaps on this account it may be, that lamp-black and oil might refift the fhock fomewhat better than the other paints.

It is remarkable, that vermillion receives the black impreffion, when painted with oil, nearly as well as when painted with water. The paper painted with white lead and oil receives alfo a black mark, but its nature is very particular. The track, when firft made, is almoft as dark as that marked on white lead painted with water; but it gradually lofes its blacknefs, and in about two hours after it appears without any darknefs, and when the painted paper is laid in a proper light appears only marked with a colourlefs track, as if made by a finger-nail.

Promifcuous Experiments.

Confidering what a ftrong fpark is obtained from the metal plate belonging to Mr. VOLTA's machine, when not the leaft fpark can be obtained from the electric plate itfelf, I was naturally induced to make ufe of the above mentioned metallic plate in difcovering the electricity of very weak electrics, which otherwife would be either in-obfervable, or fo little as not to permit its quality to be af-certained. Accordingly, by the ufe of this plate, I obtain a very fenfible electricity from the hairs of my legs and of my head, or the head of almoft any other perfon, when ftroaked.

In this manner I obtain fo ftrong fparks from the back of a cat, a hare's fkin, a rabbit's fkin, a piece of paper, or a piece of new flannel, that I can prefently charge a coated phial with either of thofe, and fo ftrongly as to pierce a hole through a card with its difcharge.

I have often obferved, that, when ftroaking a cat with one hand I hold it with the other, I feel frequent fmart pricklings on different parts of that hand which holds the animal. In thefe circumftances very pungent fparks may be drawn from the tips of the ears of the cat.

5

Smooth

Smooth glaſs rubbed with a rabbit's ſkin, dry and warm, acquires, I find, the negative electricity; but if the ſkin is cold, the glaſs is excited poſitively.

New white flannel has alſo ſuch ſtrong electric power, that ſometimes I have excited ſmooth glaſs negatively with it.

Conſidering the ſtrong electric power of new white flannel, I thought that a piece of it rolled round the globe of an electrical machine would perhaps give a ſtronger electricity on the prime conductor than the glaſs itſelf. In order to try the truth of my ſuppoſition, I tied a large piece of flannel dry and warm round the globe of the machine; and for a rubber I applied the palm of my hand, then turned the winch, firſt ſlowly, and afterwards briſkly; but, contrary to my expectation, I obſerved that the electricity at the prime conductor, although poſitive, was ſo weak, that the index of your electrometer was not moved. Surpriſed at this event, I reſolved to take off the apparatus; but I was more ſurpriſed when, on removing the flannel from the globe, the former appeared ſo ſtrongly poſitive, that it darted ſeveral ſparks to my arm and other contiguous bodies; and the latter remained ſo ſtrongly negative, that your electrometer upon the prime conductor inſtantly elevated its index to about 45°. I repeated this experiment ſeve-

ral.

ral times, and the effect was always the fame. The electricity of the flannel and of the glafs, therefore, balanced each other.

Having had occafion to coat a ten-ounce phial, I ftuck the infide coating, which was of brafs filings, with varnifh, agreeable to the directions given by fome writers on electricity. This phial remained about a week unufed; but it happened, that, whilft I was charging and difcharging it for fome experiments, I obferved that on making a difcharge it exploded with a greater noife than ufual, the cork with the wire being at the fame time blown out of the neck of it. Being intent upon the main experiments in hand, I omitted to examine the phenomenon of the phial. I replaced the cork on it, and went on charging and difcharging it again; but it had not been charged above three or four times more, when I obferved that, on making a difcharge, the varnifh that ftuck to the brafs filings was in a flame, which burned the bottom and fides of the cork confiderably, and occafioned a good deal of fmoke and flame to come out of the bottle. You will recollect, that I repeated this experiment in the prefence of yourfelf, Mr. ADAMS, and Mr. COVENTRY, when it fuccceded perfectly; but the varnifh was this time fo far burnt, that the brafs filings, which

by the combuftion had changed their colour, were almoft all dropped to the bottom of the phial.

I fhall conclude this paper with the defcription of a pocket electrometer which I have lately conftructed, and which, on feveral accounts, feems preferable to thofe of the moft fenfible fort now in ufe. The cafe and handle of the electrometer is formed by a glafs tube about three inches long, and three-tenths of an inch in diameter; half of which is covered with fealing-wax. From one extremity of this tube, that is, that without fealing-wax, a fmall loop of filk proceeds, which ferves occafionally to hang the electrometer on a pin, &c. To the other extremity of the tube, a cork is adapted, which, being cut tapering on both ends, can fit the mouth of the tube with either extremity. From one extremity of this cork two threads proceed, a little fhorter than the length of the tube, fufpending each a little cone of pith of elder. When this electrometer is to be ufed, that end of the cork which is oppofite to the threads is pufhed into the mouth of the tube, then the tube forms the infulated handle of the pith electrometer, as appears in fig. 1. When the electrometer is to be carried in the pocket, then the threads are put into the tube, and the cork ftops it, as is reprefented in fig. 2. The peculiar

advantages

advantages of this electrometer are, its convenient fmall
fize, its great fenfibility, and its continuing longer in
good order than any other I have yet feen, as you have
yourfelf experienced [a]. To preferve this electrometer
from injury, it fhould be carried in a tooth-pick cafe, or
fome other of the like fort.

(a) I have lately conftructed a portable electrometer of another kind, which
is contrived fo as not to be affected by the wind or the rain, and confequently is
very convenient to examine the electricity of the clouds out of doors in time of
thunder-ftorms: but I fhall take another opportunity to prefent to the Royal
Society a particular defcription of the fame.

Fig: 1.
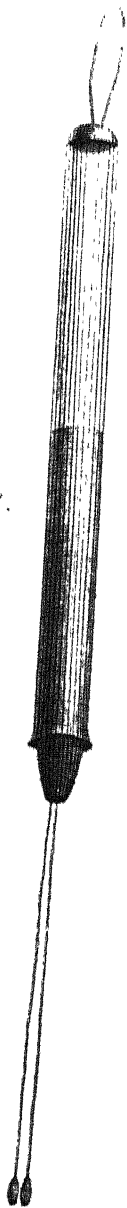
Fig: 2.

XXI. *Barometrical Observations on the Depth of the Mines in the* Hartz. *By* John Andrew de Luc, *F. R. S. In a Letter to Sir* John Pringle, *P. R. S.*

SIR,

Read March 20, 1777.

I HAVE the honour to lay before you fome obfervations on the Barometer, which I made during my fhort journey into Germany; and beg, if you think them worthy its attention, that you would communicate them to the Royal Society.

I fat out with the hopes of being able to make a fhort excurfion into the Hartz, to vifit fome of the mines there. Thefe I knew were extremely deep; and it made me very defirous to try in them my rules for meafuring heights

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Obfervations Barométriques fur la Profondeur des Mines du Hartz. *Par* Jean Andrée de Luc, *Membre de la Société Royale*, &c. *dans une Lettre au Chevalier Baronet* Pringle, *Pref. de la Soc. R.*

MONSIEUR,

J'AI l'honneur de vous faire part de quelques obfervations du Baromètre, que j'ai faites dans un petit voyage en Allemagne; vous priant de les communiquer à la Société Royale, fi vous les trouvez dignes de fon attention.

En partant pour ce voyage, j'avois l'efpérance de faire une excurfion dans le Hartz pour y vifiter quelques unes de fes mines. Je favois qu'elles étoient fort profondes; et par conféquent j'avois grande envie d'y effayer mes

heights by the Barometer, that I might know whether in thofe pits (where exhalations of all kinds fpread themfelves) the condenfations of the air follow the fame laws that they do out of them.

I nearly mifcarried in this interefting operation through an accident which had happened to my Barometer. Having lent it to a friend, when I came to examine it a little before my fetting out, I found that air had fome how got into it. Luckily I had time enough to take it to pieces and make the quickfilver boil in it; a circumftance which I only mention in order to obferve, that I fucceeded fo thoroughly in this manœuvre, that, from that time to this, the quickfilver has always adhered to the fummit of the tube as often as I have raifed it to it, juft in the fame manner as it adheres by the ebullition. It requires a fhake to make it defcend: and

fometimes

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régles pour la mefure des hauteurs par le Barométre; pour favoir fi dans ces puits, où des exhalaifons de tant d'efpéces fe répandent, les condenfations de l'air fuivroient les mêmes loix qu'au dehors.

Je faillis à manquer cette intéreffante opération par un accident arrivé à mon Barométre. Je l'avois prêté; et lorfque-je l'examinai à la veille de mon départ, je trouvai qu'on y avoit laiffé entrer de l'air. J'eus le tems heureufement de le démonter, et d'y faire bouillir le mercure: circonftance dont je ne fais mention, que pour ajouter; que je reüffis fi parfaitement dans cette opération, que dès ce moment, pendant tout mon voyage, et jufqu'à aujourd'hui, le mercure a toûjours continué de s'attacher au fommet du tube, lorfque je l'y raméne; comme il s'y attache au moment de l'ebullition: et il n'en defcend que par une

fecouffe.

fometimes the column breaks beneath the fummit, and there remain only fome lines of the quickfilver fufpended to the top.

It is for barometers cleared of air to this degree that my *formulæ* have been determined: and I had the fatisfaction to find that they anfwered in the Hartz juft as they had done upon the mountains in the neighbourhood of Geneva, where they took their origin.

Another remarkable circumftance which relates to the Barometer itfelf is as follows. Having occafion for correfponding obfervations in fome places of my rout, I applied to obfervers who had good Barometers; amongft which I met with one of Mr. DOLLOND's. Thefe Barometers I compared with my own, being well affured beforehand, that I fhould find a difference in the heights indicated, from the circumftance of their having cifterns

at

fecouffe. Quelquefois même la colomne fe romp au deffous du fommet, et il ne refte que quelques lignes de mercure fufpendu.

C'eft pour des Barométres purgés d'air à ce point, que mes formules ont été déterminées: auffi ont-elles reuffi dans le Hartz, tut comme dans les montagnes des environs de Genève, où elles ont pris naiffance.

Voici encore, Monfieur, une circonftance remarquable, qui regarde le Baromètre même. Ayant eu befoin en quelques endroits de ma route, d'obfervations correfpondentes, je m'adreffai à des amateurs, qui avoient de bons Barométres. J'en trouvai un entr' autres de M. DOLLOND. Je comparai ces Barométres au mien, étant bien affuré de trouver de la différence dans la hauteur indiquée; parcequ'ils avoient des refervoirs en bas; ce qui fait que la colomne baromè-

trique

at the bottom, which makes the barometrical column always shorter in these than it is in a plain tube in the form of a syphon, as I have shewn in my treatise on the *Modifications of the Atmosphere.* Accordingly this was the case in all these Barometers; they did all stand lower than mine, but varied from each other, according to particular circumstances, depending chiefly on the diameter of the tube, and the figure of the cistern.

In going from Hanover to the Hartz I went through Gottingen, where I did not stop then, because I wished to make use of the fine weather. I left it therefore without having settled any thing about corresponding observations of the Barometer, Professor LICHTENBERG having been kind enough to undertake the care of procuring them for me; the comparison of the instruments being deferred till my return. Professor ERXLEBEN was accordingly

<div align="right">ingly</div>

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trique y est toûjours plus courte, que dans un tuyau simple en forme de syphon; comme je l'ai expliqué dans mon Ouvrage sur les *Modifications de l'Atmosphère.* C'est aussi ce que je trouvai dans tous ces Baromètres: ils se tenoient tous plus bas que le mien; mais diversement, suivant quelques circonstances particulières, dépendantes principalement du diamètre du tube, et de la figure du reservoir.

En allant de Hanovre au Hartz, je passai par Gottingue, où je ne m'arrêtai point alors, parce que je voulois profiter du beau tems. J'en partis donc, sans avoir rien déterminé pour des observations correspondantes du Baromètre; M. le Professeur LICHTENBERG ayant bien voulu se charger du soin de m'en procurer; et renvoyant à mon retour la comparaison des instrumens. Il s'adressa pour cet effet à M. le Professeur ERXLEBEN; parce qu'il avoit un Baromètre fait d'un simple

<div align="right">tube</div>

ingly applied to by him, becaufe he had a Barometer made of a fingle bent tube upon the principle of mine. During my journey Mr. ERXLEBEN was fo good as to obferve this Barometer very frequently; and it is from his obfervations that the heights of fome of the places of the Hartz, which I fhall mention, have been determined.

At my return, I placed my Barometer near that of Mr. ERXLEBEN; and, when they were expofed to the fame temperature, there was found no difference between them.

This example, joined to all thofe with which my own experience has furnifhed me for a long time paft, makes me more and more folicitous that the lovers of natural philofophy would fix the fcale of their Barometers with cifterns (very convenient ones no doubt for common ufe) by comparing them with a Barometer made in the form

of

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tube recourbé, fur la principe du mien. M. ERXLEBEN eut la bonté d'obferver très fréquemment ce Baromètre pendant mon voyage; et c'eft d'après fes obfervations, que j'ai déterminé les hauteurs de quelques endroits du Hartz dont je ferai mention.

A mon retour j'apportai mon Baromètre auprès de celui M. ERXLEBEN; et quand ils furent réduits à la même température, il ne fe trouva entr'eux aucune différence.

Cet exemple fe joignant à tous ceux que mes propres expériences m'ont fourni depuis long tems, me fait defirer toûjours davantage que les Phyficiens veuillent bien fixer l'echelle de leurs Baromètres à refervoir (très commodes fans doute pour l'ufage ordinaire) en les comparant à un Baromètre fait en forme de fiphon;

of a fyphon, and not by any immediate meafure com-
mencing at the level of the quickfilver in the ciftern.
This is the fureft method to render exactly correfpon-
dent thofe obfervations which are made with Barometers
which one cannot compare together; at the fame time
that the barometrical height expreffed by Barometers of
this form is the only true one; that is to fay, the only
one which, after the correction for the heat, expreffes
the weight of the air, by the height of a column of
quickfilver of a given denfity, with which it is really in
equilibrio.

I fhall begin the account of my obfervations of the
Barometer in the Hartz by thofe which I made in places,
the height of which is afcertained.

Knowing that the ore is drawn up in pales from
the pits of the mines, I thought at firft that it would

be

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et non par une mefure immédiate, qui parte du niveau du mercure dans le refer-
voir. C'eft le plus fur moyen de rapporter avec exactitude les unes aux autres,
des obfervations faites avec des Barométres que l'on n'a pu comparer; en même
tems que la hauteur barométrique exprimée par les Barométres de cette forme, eft
la feule vraie; c'eft à dire la feule qui, après la correction pour la chaleur, ex-
prime le poids de l'air par la hauteur d'une colomne de mercure de denfité
donnée, avec laquelle il eft réellement en équilibre.

Je commencerai, Monfieur, le récit de mes obfervations du Barométre dans le
Hartz, par celles que j'ai faites en des lieux dont la hauteur eft connuë.

Sachant que l'on monte le minerai dans des feaux, par les puits des mines,
j'avois cru d'abord qu'il me feroit poffible de mefurer ces profondeurs au cordeau;

et

be eafy to meafure their depths with a line, and I had accordingly provided myfelf with all the neceffary implements for that purpofe; but when I arrived at Claufthal, the principal place of the King's mines, I found that thofe pits, being dug in the direction of the veins of ore, are too inclined to make fuch a mode of menfuration practicable.

At firft this gave me great concern, becaufe I had my experiments much at heart; but I was foon made eafy by Baron REDEN, captain-general of the mines. " You do not want thefe meafures," faid he, " fince it is " of much more confequence to us, than it can poffibly " be to you, to know exactly the depth of all the points " of thefe mines. Without fuch knowledge, how could " we direct ourfelves in boring from one to the other?" This confideration did in fact difpel all the fcruples which had made me defirous of meafuring the depths myfelf,

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et je m'étois muni des chofes néceffaires à cet effet. Mais lorfque je fus arrivé à Claufthal, chef lieu des mines du Roi, j'appris que ces puits, creufés dans la direction des filons, font trop inclinés pour que cette efpéce de mefure foit poffible. J'y eus d'abord beaucoup de regret; parce que j'avois fort à cœur ces expériences: mais M. le Baron DE REDEN, capitaine-gén ral des mines, me tranquillifa bientôt. " Vous n'avez pas befoin de mefurer," me dit-il, " il nous " importe bien plus qu'à vous de connoitre exactement la profondeur de tous " les points de ces mines. Sans cela, comment nous dirigerions-nous, pour " percer de l'une à l'autre?" Cette confidération en effet, fit difparoitre pleine-
ment

,myſelf, and by ſo doing gave me time for a greater number of obſervations.

The firſt of this kind were made in three mines in the environs of Clauſthal; called the Dorothea, the Caroline, and the Benedict. Mr. DE REDEN and Meſſ. HELZENER and FRIEDRICH, chief officers of the miners, went down with me; and, whilſt we were penetrating into the bowels of the mountain, Mr. LEYSER, ſyndic of the mines, and a lover of meteorological obſervations, was obſerving every quarter of an hour, at the top of the pit into which we had deſcended, a barometer and thermometer which have been ſince compared with mine.

I had obſerved my Barometer at the mouth of the Dorothea pit when we were going into the mine at half an hour after eleven in the morning; I obſerved it

at

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ment les ſcrupules qui m'avoient fait deſirer de meſurer moi-même ces profondeurs; ce qui me facilita un plus grand nombre d'obſervations.

Les prémiéres de ce genre furent dans trois mines contiguës des environs de Clauſthal; nommées la Dorothée, la Caroline, et la Bénédicte. M. DE REDEN et Meſſ. HELZENER et FRIEDRICH, prémiers officiers des mineurs, ſe donnèrent la peine d'y deſcendre avec moi: et tandis que nous nous enfonçions dans le ſein de la montagne, M. LEYSER, ſindic des mines, et amateur des obſervations météorologiques, obſerva de quart d'heure en quart d'heure, au haut du puits par lequel nous étions deſcendus, un baromètre et un thermomètre, qui furent depuis comparés aux miens.

J'avois obſervé mon Baromètre en entrant dans les mines à $11\frac{1}{2}$ h. du matin, au haut du puits de la Dorothée; je l'obſervai au fond de ce puits à $1\frac{1}{4}$ h.; au

fond

at the bottom of this pit at a quarter paſt one o'clock; at the bottom of the Caroline, at a quarter paſt three o'clock; in the loweſt ſearching gallery of the Benedict, at ſix o'clock; and again, at the mouth of the Dorothy, on our return, at ſeven o'clock. During the ſeven hours and a half that we had ſtaid in the mines, the greateſt variation of the Barometer had not exceeded a quarter of a line; and Mr. LEYSER's obſervations indicated the times when this variation had taken place.

At my return I calculated theſe obſervations, and gave the reſults to Baron REDEN, in order that the ſubterraneous Geometer might compare them with the regiſters which are kept of the depths of all theſe mines. The following are the reſults of theſe calculations, the particulars of which I likewiſe ſend you.

The

fond de celui de la Caroline à 3¼ h.; dans la galerie de recherche la plus baſſe de la Benedicte à 6 h.; et enfin je l'obſervai encore à 7 h. étant de retour à l'entrée du puits de la Dorothée. Pendant les 7½ h. que nous étions reſtés dans les mines, ſa plus grande variation avoit été d' ¼ ligne; et les obſervations de M. LEYSER me marquoient les tems où cette variation s'étoit faite.

Au retour je calculai ces obſervations, et j'en remis les réſultats à M. le Baron DE REDEN, pour les faire comparer par le Geométre ſouterrain, avec les regitres tenus de toutes les profondeurs dans les mines. Voici, Monſieur, les réſultats de ces calculs, dont j'ai l'honneur de vous envoyer auſſi les détails.

French toises.

The depth of the Dorothy pit between two fixed points,	168,96
That of the Caroline, relatively to the same point at the mouth,	170,74
That of the lowest searching gallery of the Benedict, relatively to the same point,	143,96

Mr. FRIEDRICH was appointed to give me the geometrical measures. He had been present at my observations, and found the results of them so near those measures, considering they had been furnished by a method so easy, and according to his notions so extraordinary, that he gave me a certificate in due form of the real heights, which are as follows:

The

Toises de Fr.

La profondeur du puits de la Dorothée, entre deux points fixes,   168,96
Celle du puits de la Caroline, relativement au même point d'enhaut,   170,74
Celle de la galerie de recherche la plus profonde de la Benedicte, de même,   143,96

Ce fut M. FRIEDRICH, qui fut chargé de me communiquer les mesures géométriques. Il avoit été témoin des observations; et il en trouva les résultats si près de ces mesures, pour avoir été fournis par une route si aisée et si singulière à ses yeux, qu'il m'expédia un certificat en dües formes de ces hauteurs réelles : elles étoient comme suit :

Lachters.

The depth of the Dorothy, from the points⎫
 of obfervation, ⎬ 172,31

That of the Caroline, 173,92

The gallery of the Benedict, 144,79

I could not at firft judge of the agreement of the two meafures, becaufe it was neceffary to know the relation between the Lachter and the French Toife. I had brought with me a very accurate half Toife, which we compared with the half Lachter, and found the latter fhorter than the former in the proportion of 61 to 62.

Reducing then, according to this proportion, the geometrical meafures above mentioned into French toifes, we fhall have,

The

Lachters, ou toifes
du Hartz.

Le puits de la Dorothée en partant des points des obfervations, — 172,31
Celui de la Caroline, — — — 173,92
La galerie de la Benedicte, — — — 144,79

Je ne pus pas juger d'abord du rapport des deux mefures, parcequ'il falloit connoitre celui de la Lachter avec la Toife de France. J'avois apporté avec moi une demi Toife fort exacte; nous la comparâmes à la demi Lachter, et nous trouvâmes celle-ci plus courte que la demi Toife dans le rapport de 61 à 62.

En reduifant donc fuivant ce rapport, les mefures geométriques ci-deffus en Toifes de France, nous aurons:

	French toises.
The pit of the Dorothy,	169,53
That of the Caroline,	171,12
The gallery of the Benedict,	142,42

The geometrical meafures come then very near the barometrical ones, for they only differ by

	Toifes.
In the firft obfervation,	0,57 fhort.
In the fecond,	0,38 fhort.
In the third,	1,54 excefs.

I was really furprized to have come fo near to the geometrical meafures (which, as I fhall have occafion to mention hereafter, may be looked upon as the real heights) for I had imagined, that the exhalations of all

kinds

				Toifes de Fr.
Le puits de la Dorothée,	—	—	—	169.53
Celui de la Caroline,	—	—	—	171,12
La galerie de la Benedicte,	—	—	—	142,42

Les mefures geométriques s'approchent alors de bien près des mefures baro-métriques; puifque.celles-ci différent feulement des autres favoir :

	Toife.
Dans la prémiere obfervation de,	0,57 en défaut.
Dans la feconde de,	0,38 auffi en défaut.
Dans la troifieme de,	1,54 en excès.

Je fus réellement furpris d'avoir approché de fi près des mefures geométriques; qui, comme j'aurai occafion de le dire enfuite, peuvent être regardées comme les hauteurs réelles. Car j'avois imaginé, que les exhalaifons de toute efpéce

qui

kinds in the mines muſt in ſome meaſure affect the com-
mon laws of the air's elaſticity in different degrees of
heat, if not its abſolute elaſticity. On reflecting, however,
on this ſingular conformity of the air in mines with the
external air, I ſoon found the cauſe of it in the extreme
care taken to procure a circulation of external air in the
mines, in order to prevent the pernicious effects of the
exhalations: ſo that the ſame means, which really pre-
ſerve the health of the miners in their ſubterraneous
abodes, give the air which circulates in them, and more
eſpecially that of the pits in which are the principal
currents, the properties of the external air as to baro-
metrical meaſurements. Doubtleſs this is the cauſe of
that intereſting phenomenon, as ſatisfactory for the ſe-
curity it gives to the lives of the miners, as for the ap-
plication of the laws of aërometry: this was again con-
firmed by obſervations I made ſome days after in other

.mines,

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qui ſe répandent dans les mines, devoient y altérer les loix communes de l'élaſti-
cité de l'air en différens degrés de chaleur; et peut-être ſon élaſticité abſolue.
Mais en réfléchiſſant enſuite ſur cette ſingulière conformité de l'air des mines,
avec l'air extérieur, j'en apperçus la cauſe dans le ſoin extrême qu'on prend d'y
faire circuler l'air extérieur, pour empêcher les mauvais effets des exhalaiſons.
Ainſi les mêmes moyens qui conſervent réellement la ſanté des mineurs dans
leurs demeures ſouterraines, donnent à l'air qui y circule, et ſurtout dans les
puits, où ſont les principaux courants, les propriétés de l'air extérieur dans les
meſures barométriques. C'eſt là ſans doute la cauſe de cet intéreſſant phéno-
méne, auſſi tranquilliſant ſur le ſort des mineurs, que ſur l'application des régles
d'aërométrie. Ce qui ſe confirme encore par d'autres obſervations que je fis
quelques

mines, where indeed I met with fome irregularities, but not fuch as might have been expected from barely confidering the local circumftances.

Thefe mines are in the Ramelfberg near Goflar. The ore that is chiefly extracted from them, as well as from thofe of Claufthal, is lead; but they are worked in another manner. The vein of ore, which is near eighteen toifes broad, is extremely impregnated with pyrites: infomuch that, when you heat it, the vapour of the fulphur, which difengages itfelf, burfts the ftone, which falls down in large fragments. The method then is to light great fires againft the rock; and, when they are extinguifhed, the miners affift with their inftruments the fall of the ftones, that may ftill remain fufpended.

Sulphureous vapours, therefore, conftantly difengaged from the heated ore, circulate in the caverns of the mountain,

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quelques jours après dans d'autres mines, où je trouvai quelque irrégularité, mais non point fuivant ce que les circonftances locales fembloient devoir en produire.

Ces mines font dans le Ramelfberg, près de Goflar. Elles fourniffent principalement du plomb, comme celles de Claufthal; mais on les exploite d'une autre maniére. Le filon, qui a près de 18 toifes de largeur, eft extrèmement pénètré de pyrite; tellement qu'en l'échauffant, les vapeurs du fouffre qui fe dégage, font crévaffer la pierre, qui tombe d'elle-même en grand lambeaux. On allume donc de grand feux contre le rocher; et lorfqu'ils font éteints, les mineurs aident avec des inftrumens, la chute des pierres qui font encore fufpendues.

Il fe détache donc prefque conftamment du minerai échauffé, des vapeurs fulfureufes, qui circulent dans les cavernes de la montagne, et dans les puits et
foupiraux

mountain, and in the vents and pits by which they communicate with each other. The day I entered them, was a day of reft for the miners; and there was no other fire but that which Mr. ROEDER, their principal, was kind enough to order to be lighted, that I might form an idea of this method of working mines. Notwithftanding this, I could perceive here and there fulphureous vapours, which in fome places were even ftrong enough to occafion a very troublefome fenfation of fuffocation. In fome places I felt the remainder of the heat communicated to the rock from whence thefe vapours were exhaled; and in fome caverns, where the fire had only been put out the evening before, FAHRENHEIT's thermometer rofe to 110°. But this very heat is a moft powerful ventilator, as it makes the external air circulate in thefe mines. Indeed the currents of air are fo rapid in them, that one is obliged to

have

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foupiraux par lefquels ces cavernes communiquent les unes aux autres. Le jour que j'y entrai étant un jour de repos pour les mineurs, il n'y eut de feu dans les mines, que celui que Mr. ROEDER leur chef eut l'honnêteté de faire allumer pour me donner une idée de cette exploitation. Cependant j'apperçevois ça et là des vapeurs de fouffre; et fouvent même elles étoient affez fortes, pour m'occafioner un fentiment de fuffocation très pènible. Quelquefois auffi j'éprouvois les reftes de la chaleur communiquée au rocher d'où ces vapeurs s'exhaloient: et dans quelques cavernes où le feu n'étoit éteint que dès la veille, le thermomètre de FAHRENHEIT, monta jufqu'à 110°: mais cette chaleur même eft un ventillateur très puiffant, pour faire circuler l'air extérieur dans ces mines. Auffi les courants d'air y font-ils fi rapides, qu'on eft obligé d'avoir des portes à

l'entrée

have doors at the entrance of each gallery, and some-
times many of them one after an other, without which it
would not be possible to keep any lamps lighted in
these subterraneous regions.

It is probably to this constant renewal of the air, that
the miners of Ramelsberg are indebted for the good
health they enjoy, notwithstanding the prodigious heat
they feel while they are at work, and the quantity of sul-
phur which exhales from every part; and it is likewise
probably from this cause that my barometrical observa-
tions gave me the heights more exactly than I would
have expected from these circumstances. The following
are the results of these observations, the particulars of
which you likewise have subjoined.

Height

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l'entrée de toutes les galeries; et quelquefois même plusieurs de suite; sans quoi
il ne seroit pas possible de tenir les lampes allumées dans ces souterrains.

C'est sans doute à ce renouvellement continuel de l'air, que les mineurs du
Ramelsberg doivent la bonne santé dont ils jouissent, malgré la chaleur prodi-
gieuse qu'ils éprouvent pendant le tems de leur travail, et la quantité de souffre
qui s'exhale de toute part: et c'est aussi probablement la cause, de ce que mes
observations du Barométre, me donnèrent les hauteurs, plus exactement que je
ne l'attendois d'après ces circonstances. Voici, Monsieur, les résultats de ces
observations, dont vous avez aussi les détails ci-joint.

French toifes.

Height of the gallery of Breitling, above the bottom of the pit of Kaunkuhl, } 44,41

Height of the entry of the mines, above the gallery of Breitling, } 27,04

Height of the top of the pit of Kaunkuhl, above the entry of the mines, by external obfervations, } 41,27

Depth of the pit of Kaunkuhl, meafured in three parts, one of them without the mines, } 112,72

Depth of the fame pit, determined by immediate obfervations made at the top and the bottom, } 113,13

I could not get the geometrical meafures the fame day, becaufe there was not time enough left to look for them

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Toif. de Fr.

Hauteur de la galerie de Breitling, fur le fond de puits de Kaunkühl, 44,41

Hauteur de l'entrée des mines, fur la galerie de Breitling, 27,04

Hauteur du haut du puits de Kaunkühl, fur l'entrée des mines, par des obfervations extérieures, } 41,27

Profondeur du puits de Kaunkühl, mefuré en trois portions, dont une à l'extérieur des mines, } 112,72

Profondeur du même puits, déterminée par des obfervations immédiates, au fond et au haut, } 113,13

Je ne pus pas avoir d'abord les mefures geométriques; parce qu'il ne refta pas

them then. But the day after Mr. ROEDER sent them to Mr. DE USLER, Comptroller of the treasure, who had been kind enough to accompany me to Ramelsberg, and in all my subterraneous rambles. Mr. ROEDER likewise came with us; he took notes of the places where the observations were made, and sent the following measures, which I have reduced into French toises.

French toises.

Height of the gallery of Breitling, above the bottom of the pit of Kaunkuhl,	46,86
Height of the entry into the mines, above the gallery of Breitling;	25,76
Height of the top of the pit of Kaunkuhl, above the entry of the mines,	41,32

113,94

Hence

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affez de tems pour les chercher le même jour. Mais dès le lendemain M. ROEDER les envoya à M. DE USLER, Controleur du trésor, qui avoit eu la bonté de me conduire au Ramelsberg et dans toute ma route souterraine. M. ROEDER nous y avoit accompagnés; il avoit pris note des lieux où s'étoient faites les observations; et il envoya les mesures ci-après, que j'ai seulement changées en Toises de France.

Hauteur de la galerie de Breitling, sur le fond du puits de Kaunkühl, 46,86
Hauteur de l'entrée des Mines sur la galerie de Breitling, 25,76
Hauteur du haut du puits de Kaunkühl sur l'entrée des mines, 41,32

113,94.

II

Hence it follows, that in the meafurement of the total height of the pit, the barometrical meafure differed from the geometrical one by 0,81 toife, or about $\frac{1}{140}$ in defect; that in the meafurement of a part only of this height, made without the mine, it differed only by 0,05, or about $\frac{1}{800}$, likewife in defect; but that in the two other portions of the height, taken within the mine, it differed in the one by $\frac{1}{18}$ in defect, and in the other by $\frac{1}{21}$ in excefs: and hence we obferve, that the abfolute errors are only of $2\frac{1}{2}$ toifes and $1\frac{1}{4}$ toife; and that thefe fmall differences may have arifen from fome fault in the obfervation, as well in fmall heights as in greater ones: and in this cafe, where the errors are in defect and excefs, it is probable that they arife from fome fuch caufe; and that the fulphureous vapours have little or nothing to do with them.

Having

Il refulte de là, que fur la hauteur totale du puits, la mefure barométrique a différé de la mefure geometrique, de 0,81 toife, foit d'environ $\frac{1}{140}$, en défaut; que dans la mefure d'une partie de cette hauteur, faite en dehors des mines, elle n'a différé que de 0,05, foit d'environ $\frac{1}{800}$, auffi en défaut: mais que dans les deux autres portions de la hauteur, prife fans l'intérieur de la mine, elle a différé dans l'une d'$\frac{1}{18}$ en défaut, et dans l'autre d'$\frac{1}{21}$ en excés. Sur quoi il faut remarquer; que les erreurs abfoluës ne font que de $2\frac{1}{2}$ toifes, et d'$1\frac{1}{4}$ toife; et que ces pétites différences peuvent refulter de quelque défaut dans l'obfervation, auffi bien fur de petites, que fur de grandes hauteurs. Et dans ce cas-ci, où les erreurs font en excés et en défaut, il eft bien probable qu'elles tiennent à cela, et que les vapeurs fulfureufes n'y entrent pour rien de fenfible.

Aprés

Having made thefe experiments within the mines, I was defirous of making fome in the open air, which I had foon a very agreeable opportunity of doing; for having communicated my defire to Mr. REDEN, he and Mr. RAUSCH, the principal of the fubterraneous geometers, were fo good as to be of the party. The latter had had occafion to determine moft accurately the height of two external points of the Hartz, relatively to the mines of Claufthal and Zellerfeld. Nothing more, therefore, was required but to obferve the Barometer at the entry of a certain mine, which was a fixed point, and to obferve it again at thefe two external points; one of which was about 3000 toifes horizontal diftance, beyond a fmall hill; and the other, 5000 toifes off, entirely without the Hartz.

We

Après avoir fait ces expériences dans l'intérieur des mines, je defirois beaucoup d'en faire auffi en plein air. L'ayant témoigné à M. DE REDEN, il m'en fournit un moyen très agréable; car lui même, et M. RAUSCH chef des geométres fouterrains, furent de la partie. Ce dernier avoit eu befoin, à l'occafion d'un projet de galerie d'écoulement, de déterminer avec la plus grande exactitude la hauteur de deux points extérieurs au Hartz, rélativement aux mines de Claufthal et de Zellerfeld. Il ne s'agiffoit donc que de faire l'obfervation du Barométre à l'entrée d'une certaine mine, qui étoit un point fixe; et de l'aller faire enfuite à ces deux points extérieurs; dont l'un étoit à environ 3000 toifes de diftance horizontale, au de là d'une colline, et l'autre à 5000 toifes, entiérement au dehors du Hartz.

We carried this project into execution on the 30th of October, when I found the following heights by the calculations hereunto annexed.

French toiſes.

Height of the entry of the mine, called Alte Seegen, above a certain point in the valley of Bremeke, } 102,18

Height of the entry of the ſame mine, above another point near Lasfelde, in the valley of Oſterode, } 173,81

After I had calculated theſe obſervations, Mr. RAUSCH was ſo good as to give me a profile of our rout, on which the points above mentioned were marked. Their heights, reduced to French toiſes, are as follow:

The

Nous exécutâmes ce projet le 30 Octobre; et je trouvai les hauteurs ſuivantes par les calculs ci joints de mes obſervations.

Toiſes de Fr.

Hauteur de l'entrée de la mine, nommée Alte Seegen, au deſſus d'un certain point dans la vallée de Bremeke, } 102;18

Hauteur de la même entrée de mine, au deſſus d'un autre point près de Lasfelde, dans la vallée d'Oſterode, } 173,81

Après que j'eus calculé ces obſervations, M. RAUSCH eut la bonté de me donner un profil de la route que nous venions de faire, où les points ci-deſſus étoient marqués. Leur hauteur, reduite en Toiſes de France, eſt comme ſuit.

Be

The point of the valley of Bremeke below Alte Seegen, } 100,85

That of the vale of Osterode,   173,56

Consequently, one of these barometrical measures, taken in open air, was found to agree very nearly with the geometrical measure; and the other differs only $1\frac{1}{3}$ toise in excess.

The only thing that remained to be enquired into was, whether the geometrical measures could really be relied on; but I soon found that I might trust to the importance they were of to the miners, as well as to the daily experience of the truth of them; although they be made in so very singular a manner, that one does stand in need of this experience, to be persuaded of their exactness.

A twisted

Le point de la vallée de Bremeke, au dessous d'Alte Seegen,   100,85
Celui de la vallée d'Osterode,   173,56

Ainsi, l'une de ces mesures barométriques faites en plein air, s'est trouvée presqu' entiérement semblable à la mesure geométrique; l'autre n'en diffère que d'une toise et un tier en excès.

Il ne s'agissoit plus que d'examiner, si les mésures geométriques étoient vraiment dignes de confiance. Mais je vis bientôt, que je pouvois me reposer à cet égard sur l'importance dont il est pour les mineurs qu'elles le soyent, et sur l'expérience qui les vérifie tous les jours. Cependant elles s'exécutent d'une manière si singuliére, qu'il faut réellement cette expérience, pour se perfuader qu'elles font exactes.

Un

A twifted brafs· wire five toifes long, two puncheons, a femi-circle, and a compafs, are all the inftruments made ufe of by the fubterraneous Geometer. By means of his two puncheons, he extends his wire in the direction of the way which he is meafuring: and by practice he acquires a habit of always ftretching it·to the fame degree. His fe-mi-circle, which is very light, being fufpended at the mid-dle of the wire, fhews him its inclination. By this means he has a right-angled triangle, of which the hypothenufe and angle at the bafe are known. He has confequently the vertical height· and horizontal diftance gone over. After this he fufpends his compafs to the.wire, in order to find out its declination, and confequently the direction of his horizontal line. It is in this manner that he draws the plan and fection of thefe fubterraneous labyrinths. It is likewife by this means that he goes over hills and vales,, in

Un fil de leton tordu de 5 toifes, deux poinçons, un demi-cercle, et une bouffole, font tous les inftrumens du Geométre fouterrain. Il étend fon fil, par le moyen de fes deux poinçons, dans la direction du trajet qu'il mefure; l'habitude le lui fait tendre toûjours au même degré. Son demi-cercle, qui eft fort léger, étant fufpendu au milieu de ce fil, lui en montre l'inclinaifon; il a par ce moyen un triangle rectangle, dont l'hypothenufe et l'angle fur la bafe lui font connus:: il a donc la hauteur verticale et la diftance horizontale parcouruës. Il fufpend enfuite fa bouffole au même fil, pour en connoitre la déclinaifon, et par confé-quent la direction de fa ligne horizontale. C'eft ainfi qu'il tire le plan et la coupe de ces labyrinthes fouterrains: et c'eft ainfi encore qu'il va chercher au

in order to determine points corresponding to his pits and galleries.

But is this a method that may safely be depended upon? The fact answers, and saves us the trouble of long reasonings. A miner, solely upon the faith of his Geometer, and in the absolute obscurity of the entrails of the earth, undertakes a labour that is to cost him years, in daily boring through a rock. Another miner sets out to meet him from some other mine, or from without. At the end of a determined measure, the Gnomes begin to hear each other, and at length they meet. I have observed some of these points of rencounter in the galleries; it is sometimes difficult to perceive the small winding which has been necessary for their meeting end to end.

It

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dehors, à travers les vallées et les collines, des points correspondants à ses galeries et à ses puits.

Est-ce donc là une méthode dans laquelle on puisse vraiment prendre confiance? Le fait parle ici, et épargne les raisonnemens. Le mineur, sur la foy de son Géomètre, s'avanture à entreprendre, dans l'absolue nuit des entrailles de la terre, un travail qui lui coutera des années, en perçant journellement le rocher. On vient à sa rencontre, de quelqu'autre mine, ou du dehors. Au bout de la mesure determinée, nos Gnomes viennent à s'entendre; et enfin ils se trouvent. J'ai vu plusieurs de ces points de rencontre dans les galeries; on a peine quelquefois à appercevoir le petit évasement qu'il a fallu faire, pour qu'elles se ▪▪▪ rent bout à bout.

It remains, SIR, that I communicate to you some other barometrical obfervations, not verified by geometrical furvey; by which I have determined the height of fome points of the Hartz relatively to the plain, and chiefly the higheſt point.

This greateſt elevation, called the Blockſberg or Brocken, is ſituated in the eſtate of Count DE VERNIGUE-RODE. It was my firſt excurſion on my arrival at the Hartz, and I made it in the company of Mr. REDEN. We ſat out from Clausthal at ten o'clock, and arrived at Oder-bruck, a hamlet ſituated at the foot of the Brocken, at half an hour after two in the morning. We intended ſetting out at day-break, in order to reach the top of the mountain by ſun-riſe, becauſe that is the moſt favourable moment for ſeeing the great extent of country, which is diſcoverable from this eminence; the vapours, which by degrees tarniſh the picture, not having yet riſen. On this

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Il me reſte à vous communiquer, Monſieur, d'autres meſures barométriques, non vérifiées, par leſqu'elles j'ai déterminé la hauteur de quelques points du Hartz relativement à la plaine, et principalement le plus haut point.

Cette ſommité la plus élevée, nommée le Blockſberg ou Brocken, eſt ſituée dans les terres de M. le Comte DE VERNIGUERODE. Cet fut ma premiére courſe lorſque j'arrivai au Hartz; et M. le Baron DE REDEN la fit déja avec moi. Nous partimes à 10 h. du ſoir de Clausthaul, et nous arrivâmes à $2\frac{1}{2}$ h. du matin à Oder-brucke, hameau ſitué au pied du Brocken. Notre intention étoit de nous mettre en marche à la pointe du jour, pour arriver au lever du ſoleil au ſommet de la montagne; parceque c'eſt le moment le plus favorable pour voir l'immenſe pays que l'on decouvre de cette hauteur: les vapeurs qui peu à peu terniſſent le

this occasion, however, other vapours rendered our noc-
turnal course ineffectual.   Clouds rested on the top of
the mountain, and we hesitated a long time whether or
not we should put off the party to another day.   At length
a ray of hope having broke forth at Oder-brucke, we de-
termined to set out for the mountain.   It was then nine
o'clock in the morning, and, had we had fair weather, an
hour would have been sufficient to have carried us to the
top; but the clouds growing thicker and thicker, we lost
our way, notwithstanding we had guides; and, had it not
been for a map and compass which M. DE REDEN had
provided himself with, we should not probably have ar-
rived at the top of the Brocken that day, having been for
some hours wandering round it, without drawing nearer
to it.   We did at length reach it at noon, being ourselves
as well as the mountain covered with isicles.   The wind

<div align="right">was</div>

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tableau, n'étant pas encore élevées.   Mais d'autres vapeurs rendirent inutile
notre course de nuit: les nuages s'emparérent du haut des montagnes; et long-
tems nous hésitâmes si nous ne renverrions pas la partie à un autre jour.   Mais
enfin un rayon d'espérance ayant lui à Oder-brucke, nous nous déterminâmes à
partir pour la montagne.   Il etoit 9 h. du matin; et une heure eût suffit pour nous
rendre au sommet, si nous avions eu beau tems.   Mais les nuages s'etant épaissis
toûjours d'avantage, nous nous égarâmes malgré nos guides; et sans une boussole
et une carte de ces montagnes dont M. DE REDEN avoit eu la précaution de se
munir, nous ne serions peut-être pas arrivés de tout le jour au Brocken, autour
duquel nous tournions, sans le trouver.   Il étoit midy lorsqu'enfin nous l'attei-
gnimes; étant nous mêmes couvers de verglas, comme toute la montagne.   Il

<div align="right">faisoit</div>

was very high, the thermometer ftood at $31\frac{1}{2}$ of FAHREN-HEIT; and the clouds, chafed by the wind, covered every thing with a cruft of ice that formed itfelf perceptibly to the eye.

I had taken with me a new hygrometer, conftructed upon the principles of that I had the honour to prefent to the Royal Society four years ago, but corrected in many things where experience had taught me better. I was in hopes, by taking it to the Hartz, of being able to repeat the experiment of the great drynefs of the fuperior regions of the air, which I had made formerly on the Alps; but it happened quite otherwife, and I was not forry for the contraft. Being arrived at the top of the Brocken, I fuf-pended my hygrometer on the outfide of a fmall hut, which M. DE VERNIGUERODE has had the humanity to erect for the benefit of fuch obfervers as fhould

come

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faifoit un vent affez fort, le thermométre étoit à $31\frac{1}{2}$ de FAHRENHEIT; et les nuages chariés par le vent, couvroient tout d'une croute de glace, qui fe formoit à vuë d'oeil.

J'avois porté avec moi un nouvel hygrométre, conftruit fur les principes de celui que j'ai eu l'honneur de préfenter à la Societé Royale il y a quatre ans; mais où j'ai corrigé plufieurs des défauts que l'experience m'a déja fait découvrir dans ma prémiére tentative. . J'efpérois en le portant au Hartz, de répéter l'ex-périence de grande féchereffe des couches fupérieures de l'air, que j'avois faite ci devant fur l'une des fommités des Alpes. Mais il en arriva tout autrement: et je ne fus pas fâché du contrafte. Arrivés donc enfin au fommet du Brocken, je fufpendis mon hygrométre au dehors d'une petite hute, que M. DE VERNIGUE-RODE a eu l'humanité de faire bâtir pour fervir de refuge aux curieux dans le

mauvais

come there in bad weather. In an inſtant the ivory, of which this new inſtrument as well as the former is conſtructed, was covered with iſicles; and what merits ſome attention as to the matter of the *humor*, the hygrometer was by that means brought very nearly down to the point of extreme humidity.

I ſhall not here detain you with accounts of ſeveral other obſervations made with this inſtrument in the courſe of my journey. I am much better pleaſed with it than with the firſt, for many reaſons; however, I have not got over all the difficulties, but luckily alſo I have not exhauſted all the reſources.

I likewiſe obſerved the Barometer on this eminence; I had done it at ſetting out from Oder-brucke, and did it again on my return. I had likewiſe correſponding obſervations at Clauſthal, and during the whole of my journey

I had

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mauvais tems. En un inſtant l'yvoire, dont ce nouvel inſtrument eſt fait comme le prémier, fut couvert d'une couche de verglas; et ce qui mérite quelque attention dans la matiere de *l'humor*, l'hygrométre fut réduit par là à très peu près au point de l'humidité extrême.

Je ne m'arràterai pas ici ſur quelques autres obſervations que j'ai faites avec cet inſtrument pendant mon voyage. J'en ai été plus content que du prémier à divers égards. Cependant je ne ſuis pas au bout des difficultés: mais heureuſement non plus, je ne ſuis pas au bout des reſources.

Je fis auſſi l'obſervation du barométre ſur cette hauteur. Je l'avois faite en partant d'Oder-brucke, et je la fis au retour. J'avois auſſi des obſervations correſpondantes à Clauſthal: j'en eus pendant tout mon voyage de journaillères, à

I had daily ones regiftered at Claufthal, at Gottingen, and at Hanover. From all thefe obfervations I computed the height of Oder-brucke above all the above-mentioned places, by calculations which I have annexed.

French toifes.

The fmall hut, at the fummit of the Brocken above Oder-brucke, } 172,93

Oder-brucke above Claufthal, · 91,39

Claufthal above Gottingen by 17 obfervations correfponding as to time, between Profeffor ERXLEBEN and myfelf, } 210,21

Gottingen above Hanover by 16 fimilar obfervations, between Mr. ERXLEBEN and M. DE HINUBER, } 56,45

Total elevation of the Brocken above Hanover, 530,98

It

Claufthal, à Gottingue, et à Hanovre. De toutes ces obfervations j'ai conclu la hauteur du Brocken fur tous les lieux ci-deffus, par les calculs ci-joints, dont voici l'extrait.

Toifes de Fr.

La maifonette au fommet du Brocken, fur Oder-brucke, 172,93

Oder-brucke fur Claufthal, 91,39

Claufthal fur Gottingue, par 17 obfervations correfpondantes pour le tems, entre M. le Profeffeur ERXLEBEN et moi, } 210,21

Gottingue fur Hanovre, par 16 femblables obfervations entre M. ERXLEBEN et M. DE HINUBER, } 56,45

Hauteur totale du Brocken fur Hanovre, 530,98

It will be eafy to come at the elevation of Hanover above the level of the fea, in order to compleat this meafurement: correfponding obfervations of the Barometer will be fufficient for that purpofe. In the mean time it is eafy to eftimate either by the Barometer itfelf (the mean height of which during the month of October was 30,1 Englifh inches on a fecond floor) or by the fmall declivity of the rivers between Hanover and the fea, that the elevation of Hanover above that level is not very confiderable.

Such, sir, are the moft interefting obfervations of this kind which I have been able to make in the Hartz. It appears to me, that they are of a nature that fhould encourage us to examine more and more into the phyfical principles on which they depend: principles, the extenfive confequences of which promife new fteps in our inveftigation of nature, not only upon our globe but alfo in the heavens.

A De-

Il fera aifé de favoir la hauteur de Hanovre fur le niveau de la mer, pour completter cette mefure. Des obfervations correfpondantes du barométre, fuffiront pour cela. Mais en attendant il eft aifé de juger, foit par le barométre lui même, dont la hauteur moyenne le matin, pendant le mois d'Octobre, fut 30,1 pouces Anglois à un feconde etage; foit par le peu de pente des riviéres jufques à la mer; que l'élevation de Hanovre au deffus de fon niveau n'eft pas bien grande.

Voilà, monfieur, les obfervations les plus intéreffantes de ce genre, que j'aie faites dans le Hartz. Il me femble qu'elles font propres à donner le courage d'examiner de plus près tous les principes phyfiques fur lefquels elles fe fondent; principes, dont les conféquences très étenduës, nous promettent de nouveaux pas dans l'étude de la nature, non feulement fur la terre, mais dans le ciel.

Defcription

A Detail and Calculations of Barometrical Obfervations made at the Hartz *in* October 1776.

Obfervations in the Mines of Claufthal. —

October 26, 1776, at twenty minutes paft eleven in the morning, I obferved my Barometer at the entrance of the pit of the mine called Dorothy, and found it (after the allowance made for the heat both in this and all the following obfervations) 26 in. 4. l. $\frac{1}{16}$ French meafure.

Syndic LEYSER's Barometer being placed near mine, and obferved every quarter of an hour throughout the whole day, fell a quarter of a line, between half and three quarters of an hour after twelve, and did not vary fenfibly till four o'clock. It then had a tendency to rife, for it was ftationary, though the Thermometer fell; fo that, on

coming

Defcription et Calculs des Obfervations du Baromètre faites au Hartz, *en* October 1776.

Obfervations dans les Mines de Claufthal.

Le 26 Octobre, 1776, à 11 h. 20 m. du matin, j'obfervai mon baromètre à l'entrée du puits de la mine nommée Dorothée; et je le trouvai (la correction étant faite pour la chaleur ici, et dans toutes les obfervations fuivantes) 26 p. 4 l. $\frac{1}{16}$ de France.

Le baromètre le M. le Sindic LEYSER étant placé auprès du mien, et obfervé enfuite pendant tout le jour de quart d'heure en quart d'heure, defcendit d' $\frac{1}{4}$ de ligne de midi $\frac{1}{2}$ à midi $\frac{3}{4}$; et ne varia plus fenfiblement jufqu'à 4 heures. Alors il tendit à remonter; car il fut ftationaire quoique le thermomètre baiffait:

tellement

coming out of the mines, my Barometer, after the correction for the Thermometer, was much at the same point as when I went in.

I had not made any obfervations in the mines before this firft variation, fo I will look upon 26 in. 3 l. $\frac{13}{16}$ as being the height of the Barometer at the top of the pit of the Dorothy during my obfervations in the internal part of the mines till three o'clock. But at fix o'clock I fhall fuppofe it at 26 in. $3\frac{15}{16}$.

At

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tellement qu'au fortir des mines mon baromètre, après la correction pour le thermomètre, fe trouva prefque au même point que lorfque nous y étions entrés.

Je n'avois point encore obfervé dans les mines avant le tems de cette prémiere variation : ainfi je regarderai 26 p. 3 l. $\frac{13}{16}$ comme étant la hauteur du baromètre au haut du puits de la Dorothée pendant mes obfervations dans l'intérieur des mines jufqu'à $3\frac{1}{2}$ h. Mais à 6 h. je la fuppoferai à 26 p. $3\frac{15}{16}$.

In. L. 16 de Lig. Log.

At 1¼ h. at two feet from the bottom of the well of the Dorothy,   27 4 $\frac{11}{16}$ = 5260   37209,857

At the top of the pit,   26 3 13 = 5053   37035,493

Difference, — —   174,364

The thermometer on the outside of the building which is over the mine was at −22 of my fcale for the dilatations of the air; but this external temperature reached a very little way into the mine. At the bottom of the pit the thermometer was −15. I reckon the mean temperature −15½, and confequently I am to deduct $\frac{31}{1000}$ from the height found by the log, — —   —   5,405

Depth of the Dorothy pit in French toifes,   168,959

At

                     P. L.   16 de Lig.   Log.

A 1¼ h. à 2 pieds du fond actuel du puits de la Dorothée, — —   27 4 $\frac{11}{16}$ = 5260   37209,857

Au haut du puits, — —   26 3 13 = 5053   37035,493

Difference, — —   —   174,364

Le thermométre au dehors du batiment qui couvre la mine étoit à −22 de mon echelle pour les dilatations de l'air. Mais cette température extérieure s'étendoit bien peu avant dans la mine. Au fond du puits, le thermométre étoit −15. Je fuppofe la température moyenne −15½, et en confequence je dois deduire $\frac{31}{1000}$ de la hauteur trouvée par les log, — —   —   5,405

Profondeur du puits de la Dorothée, en toifes de France, — 168.959

<div align="right">In. L.　.　16th of Lines.　Log.</div>

At 3½ h. at a foot and a half from the bottom of the laſt ladder of the Caroline,　- 27　4　13 = 5261　37210,683

At the top of the pit of the Dorothy,　- 26　3　13 = 5053　37035,493

Difference,　——　——　175,190

In the pit of this mine my thermometer ſtood at −11 of the ſcale for the dilatations of the air. But the outſide thermometer being then at −23¾, I reckon the mean temperature at −13, and conſequently I deduct $\frac{26}{1000}$,　- 4,455

The bottom of the pit of the Caroline below the entrance of the pit of the Dorothy,　——　——　——　—— 170,735

<div align="right">At</div>

<div align="right">P. L.　16 de Lig.　Log.</div>

A 3½ h. a 1½ pied du bas de la dern. echelle de la Caroline,　——　—— 27　4　13 = 5261　37210,683

Au haut du puits de la Dorothée,　—— 26　3　13 = 5053　37035,493

Difference,　——　——　—— 175,190

Dans le puits de cette mine, mon thermométre ſe tenoit à −11 de l'echelle pour les dilatations de l'air. Mais le thermométre étant alors au dehors à −23¾, j'éſtime la temperature moyenne à −13. Et conſéquemment je deduis $\frac{26}{1000}$, 4,455

Le fond du puits de la Caroline au deſſous de l'éntrée du puits de la Dorothée,　——　——　——　—— 170,735

<div align="right">A 6 h.</div>

In. L.   16th of Lines.   Log.

At 6 o'clock at the bottom of the loweſt ſearching gallery of the Benedict, —— } $27\ 2\ 14 = 5230$ $37185,017$

At the top of the pit of the Dorothy, - } $26\ 3\ 15 = 5055$ $37037,212$

Difference, $147,805$

At the place of obſervation in the mine the thermometer was at $-8\frac{3}{4}$, higher by $2\frac{1}{4}$ than at the pit of the Caroline. But as this gallery communicates with the ſame pit, and that conſequently the column of air contained in it is part of that which weighs upon the air of the gallery, I am only to eſtimate the temperature of the pit. I therefore deduct $\frac{26}{1000}$, as in the preceding obſervation, —— } $3,843$

The gallery, &c. of the Benedict, below the entrance of the Dorothy, —— } $143,962$

P. L.   16 de Lig.   Log.

A 6 h. au fond de la galerie de recherche la plus baſſe de la Bénédicte, —— } $27\ 2\ 14 = 5230$ $37185,017$

Au haut du puits de la Dorothée, —— $26\ 3\ 15 = 5055$ $37037,212$

Difference, $147,805$

Au lieu de l'obſervation dans la mine, le thermométre étoit à $-8\frac{3}{4}$, plus haut de $2\frac{1}{4}$ qu'au puits de la Caroline. Mais comme cette galerie communique au même puits, et que par conſequent la colonne d'air qu'il renferme, fait partie de celle qui péſe ſur l'air de la galerie, je ne dois avoir égard qu'à la temperature du puits. Je deduis donc $\frac{26}{1000}$ comme dans l'obſervation précédente, —— } $3,843$

La galerie, &c. de la Bénédicte, au deſſous l'entrée de la Dorothée, $143,962$

Obfervations at the Ramelfberg.

On the 28th of October, at $9\frac{3}{4}$ h. I obferved the barometer under the cover of the entrance of the mines, two feet and a half above the aperture of the pit, and found it at   ——   } 27 2 3

At $2\frac{3}{4}$ h. when I left the mines, it was at - 27 2 8

Confequently it had rifen $\frac{5}{16}$ths of a line in five hours; whence, not having made any regular obferva-tions during this interval, I will fuppofe the variation to have been equable, that is, $\frac{1}{16}$th by the hour, during the time I ftayed in the mines.

At

Obfervations faites au Ramelfberg.

Le 28 Octobre, à $9\frac{3}{4}$ h. du matin, J'obfervai le Baromètre fous le couvert de l'entrée des mines, $2\frac{1}{2}$ pieds au deffus de l'ouverture du puits, et je le trouvai à   ——   ——   —— } 27 2 3

A $2\frac{3}{4}$ h. au fortir des mines, il fe trouva à   ——   —— 27 2 8

Il avoit donc monté de $\frac{5}{16}$ de lignes en 5 heures; et n'ayant pas eu d'obferva-tions fuivies pendant cet intervalle, je fuppoferai la variation graduelle, et ainfi d' $\frac{1}{16}$ par heure, durant le tems que je reftai dans les mines.

	In. L.	16th of Lines.	Log.
At 11½ h. at the bottom of the pit of Kaunkuhl, —	} 27 7	14 = 5310	37250,945
At 12½ h. at the entrance of the gallery of Breitling 27 4 8; confequently an hour before at 11½, -	} 27 4	7 = 5255	37205,729

Difference, — —	45,218
At the bottom of the pit, therm. — −5 Near the gallery, - −13 } −9 mean, deduct therefore $\frac{18}{1000}$, }	,813

Height of the gallery of Breitling above the bottom of the pit of Kaunkuhl, — }	44,405

At

	P. L.	16 de Lig.	Log,
A 11½ h. au fond puits de Kaunkühl, — A midi ½, à l'entrée de la galerie de Breitling 27 4 8; donc 1 h. plus tòt, foit 11½ h. — }	27 7 27 4	14 = 5310 7 = 5255	37250,945 37205,727

Difference, — —	45,218
Au fond du puits, therm.—5 Auprès de la galerie, —13 } −9 moyen, donc $\frac{18}{1000}$ à deduire,	,813

Hauteur de la galerie de Breitling, fur le fond du puits de Kaunkühl, 44,405

At 12¼ h. at the entrance of the gallery of Breitling, near the pit, — — } 27 4 8 = 5226   37206,554

Under cover of the mines, at the same time, — — } 27 2 6 = 5222   37178,369

Difference, — — 28,185

Therm. in the pit — 13, deduct therefore $\frac{26}{1000}$,   732

27,453

Deduct the elevation of the barometer above the entrance of the mines, — } 416

The height of the entry of the mines above the gallery of Breitling, — — } 27,037

At

P. L.   16 de Lig.   Log.

A midi ¼ à l'entrée de la galerie de Breitling, près du puits, — — } 27 4 8 = 5226   37206,554

Sous le couvert des mines, à cette heure là — 27 2 6 = 5222   37178,369

Difference, — — — 28,185

Thermométre dans le puits —13; donc $\frac{26}{1000}$ à deduire, — ,732

27,453

A deduire ce dont le baromètre étoit plus haut que l'entrée des mines, ,416

Hauteur de l'entrée des mines, fur la galerie de Breitling, — 27,037

In.　L.　16th of Lines.　Log.

At 2¾ h. under the
cover of the mines, } 27　2　8 = 5224.　37180,032.

At 2½ h. at the top
of the pit of Kaun-
kuhl, — — } 26　11　6 = 5174　37138,264.

Difference, — — — 41,768

These two obfervations were made on the
outfide of the mines, the heat of the air being
— 11, deduct therefore $\frac{22}{1000}$, — — } 419.

40,850.

Add the elevation of the barometer under
the cover, above the entrance of the mines, } 918.

Height of the top of the pit of Kaunkuhl
above the entrance of the mines, — } 41,266

At

P.　L.　16 de Lig.　Log.

A 2¾ h. fous le couvert des mines, — 27　2　8 = 5224.　37180,032
A 2½ h. au haut du puits de Kaunkühl, 26　11.6 = 5174　37138,264.

Difference, — — 41,768
Ces deux obfervations furent faites au dehors des mines, la chaleur
de l'air étant — 11; donc $\frac{22}{1000}$ à deduire, — — } ,918.

40,850.
A ajouter ce dont le baromètre fous le couvert, étoit au deffus de
l'entrée des mines, — — — — } ,416.

Hauteur du haut du puits de Kaunkühl, fur l'entrée des mines, 41,266.

	In.	L.	16th of Lines.	Log.

At 11½ h. at the bottom of the pit of Kaunkuhl as above,   27   7   14=5301   37250,945

At 2½ h. at the top of the pit 26 11 6, confequently at 11½ h.   26 11   3=5171   37135,745

Difference,   — —   115,200

At the bottom of the pit, therm.   −5   −9, confequently $\frac{18}{1000}$ to deduct,   2,074
At the top,   −13

Total depth of the pit of Kaunkuhl, — 113,126

Obfer-

	P. L.	16 de Lig.	Log.

A 11½ h. au fond du puits de Kaunkühl, comme ci-deffus, — —   27 7 14 = 5310   37250,945

A 2½ h. au haut du puits 26 11 6, donc à 11½ h. — —   26 11   3=5171   37135,745

Difference, — — 115,200

Au fond du puits, therm. −5
Au haut, −13   −9, donc $\frac{18}{1000}$ à deduire, — 2,074

Profondeur totale du puits de Kaunkühl, — — 113,126

Obſervations at two Points geometrically determined on the outſide of the Mountain, relatively to the Mine of Alte Seegen.

	In.	L.	
On the 30th of October, at 10 in the morning, I obſerved the barometer under the cover of the mine called Alte Seegen near Zellerfelt, and found it at — —	21	71	2
At half an hour paſt ſeven at night, at my return from theſe obſervations, it was at	26	6	8

Conſequently in nine hours and a half the barometer fell $\frac{10}{16}$ths of a line; and by obſervations made by Syndic LEYSER, this fall was gradual.

At

Obſervations en deux Points déte minés géométriquement à l'extérieur de la Montagne, relativement à la Mine d'Alte-Seegen.

	P.	L.	
Le 30 Octobre, à 10 h. du matin, j'obſervai le baromètre ſous le couvert de la mine nommée Alte Seegen, ſituée près de Zellerfeld, et je le trouvai à — — — —	27	71	2
A $7\frac{1}{2}$ h. du ſoir, au retour des obſervations ci après, il ſe trouva à	26	6	8

Ainſi en $9\frac{1}{2}$ h. le baromètre baiſſa de $\frac{10}{16}$ de ligne. Et par des obſervations faites d'heure en heure par M. le Sindic LEYSER, cet abbaiſſement fut graduel.

	In. L.	16th of Lines.	Log.

At 12 h. in the valley of Bremeke, half a toife lower than a certain determined point, 27 3 2 = 5234 37188,357

At Alte Seegen, two hours after the firſt obſervation, —— 27 7 0 = 5104 37079,107

Difference, —— —— 109,230

Heat of the air − 30, conſequently $\frac{60}{1000}$ to deduct 6,554

102,676

Deduct the half toiſe above mentioned, —— 500

French toiſes, —— 102,176

At

	P. L.	16 de Lig.	Log.

A midi, dans la vallée de Bremeke, $\frac{1}{2}$ toiſe plus bas qu'un certain point déterminé, 27 3 2 = 5234 37188,337

A Alte Seegen, 2 h. aprés la prémiere obſervation, —— 26 7 0 = 5104 37079,107

Difference, —— —— 109,230

Chaleur de l'air —30; donc $\frac{60}{1000}$ à deduire, —— —— 6,554

102,676

A deduire la demi toiſe ci-deſſus, —— —— ,500

Toiſes de France, —— —— 102,176

A 1¾ h.

	In.	L.	16th of Lines.	Log.

At $1\frac{3}{4}$ h. at Lasfelde without the Hartz, in the valley of Ofterode, at a determined point, $\}$ 27 8 10 = 5322   37260,749

At Alte Seegen, about four hours after the firft obfervation, $\}$ 27 8 10 = 5102   37077,405

$$183,344$$

Heat of the air – 26, confequently $\frac{52}{1000}$ to deduct   9,534

French toifes,   —   —   173,810

Obfer-

	P. L.	16 de Lig.	Log.

A $1\frac{3}{4}$ h. à Lasffelde, hors du Hartz, vallée d'Ofterode, à un point déterminé,   — $\}$ 27 8 10 = 5322   37260,749

A Alte Seegen environ 4 h. après la prèmiere obfervation,   —   — $\}$ 26 6 14 = 5102   37077,405

$$183,344$$

Chaleur de l'air —26; donc $\frac{52}{1000}$ à deduire,   —   —   9,534

Toifes de France,   —   —   173,810

Observations to determine the height of the Brocken *with respect to* Gottingen *and* Hanover.

The 25th of October, on setting out from Oder-brucke for the Brocken, at three quarters past eight in the morning, ⎱ In. L. 25 9 8 On the first floor in the Inn.

In returning, at 3¼ h. ── ── 25 8 14

Difference, ── ── 0 0 10

Observed at the Brocken at Noon and at 1 o'clock, the mean, at about 12½ h. makes nearly half the space of time above mentioned; consequently the height of the barometer at Oder-brucke at that hour may be reckoned at 25 9 3.

Sindic

Observations pour déterminer la hauteur du Brocken *relativement à* Gottingue *et à* Hanovre.

P. L.

Le 25 Octobre, en partant d'Oder-brucke pour le Brocken, à 8¾ h. du matin, ── ── 25 9 8 Au prémier étage du Cabaret.

En revenant, à 3¼ h. ── ── 25 8 14

Difference, ── ── 0 0 10

Observé au Brocken à midi et à 1 heure. Le milieu, midi ½, fait à peu près la moitié de l'intervalle de tems ci-dessus; et par conséquent on peut évaluér la hauteur du baromètre à Oder-brucke à cette heure la, 25 9 3.

M. le Sindic

Sindic LEYSER obferved the barometer at his houfe at Claufthal, in the morning and at noon; when inftead of falling, as at Oder-brucke, it rofe half a line, and remained at that point till the evening. The only caufe to which I can afcribe this difference between his obfervation and mine, is the increafe of heat in his room: but there was no obfervation with the thermometer to correct the effect of this caufe; I fhall therefore content myfelf with my own obfervation for this meafure, though I employ Mr. LEYSER's for the following one.

M. le Sindic LEYSER obferva le baromètre chez lui à Claufthal le matin et à midi: et au lieu de baiffer, comme à Oder-brucke, il monta de $\frac{1}{2}$ ligne, et refta à ce point jufqu'au foir. Je ne faurois attribuer cette différence entre l'ob--fervation de M. LEYSER et la mienne, qu'à ce que la chaleur augmenta dans fa chambre: mais il n'y eut point d'obfervation du thermométre pour corriger l'effet de cette caufe. Je m'en tiens donc à mon obfervation feule pour cette mefure-ci; quoique j'employe celle de M. LEYSER pour la fuivante.

	In.	L.	16th of Lines.	Log.

At 12½ h. the baro-meter at Oder-brucke muſt have been at, } 25 9 3 = 4947   36943,419

At the Brocken,
at 12 h.   23 8 5
at 1h. 5 m. 24 8 1 } mean 29 8 3 = 4739   36756,867

Difference,   —   —   186,552

On ſetting out from Oder-brucke the heat of the air was at - } $-34\frac{1}{2}$ } -33 $\begin{array}{l}-36\frac{1}{2},\\ \text{conſeq.}\\ \frac{73}{1000}\text{ to}\\ \text{deduct,}\end{array}$ } 13,618

At our return, - $-31\frac{1}{2}$
At the Brocken, — -40

Height of the Brocken above Oder-brucke,   172,934

By

	P. L.	16 de Lig.	Log.

A midi ½ le baromètre devoit être à Oder-brucke, 25 9   3 = 4947   36943,419

Au Brocken, { à midi   24 8 5
à 1 h. 5 m. 24 8 1 } moyen,   24 8   3 = 4739   36756,867

Difference,   — —   186,552

En partant d'Oder-brucke, la chaleur de l'air etoit, — — } $-34\frac{1}{2}$ } -33 $\begin{array}{l}-36\frac{1}{2}\\ \text{donc }\frac{73}{1000}\\ \text{à deduire,}\end{array}$ } 13,618

Au retour, — — $-31\frac{1}{2}$
Au Brocken, — — — -40

Hauteur du Brocken ſur Oder-brucke,   — —   172,934

Par

By the above mentioned obfervations of Mr. DE LEY-
SER's, and by the comparifon between his barometer
and mine, I have reafon to conjecture, that at the time of
the mean obfervation of the barometer at Oder-brucke,

	In.	L.	16th of Line.	Log.
it ftood at Claufthal at,	26	4　3 = 5059		37040,647
Mean height ob- ferved at Oder-brucke,	25	9　3 = 4947		36943,419

Difference, — —	97,228

The mean heat of the air muft have been a
little greater than at Oder-brucke; I compute
it at — 30, confequently $\frac{60}{1000}$ to deduct,　-　　　　5,834

Height of Oder-brucke over the firft floor
of Mr. DE LEYSER's houfe at Claufthal, or
what comes to the fame nearly, above the firft-　91,394
floor of the Crown Inn, where I lodged,　-

The

Le

The mean term betwixt 17 obfervations of the baro-
meter, made at Gottingen from the 24th of October to
the 1ft of November, by Profeffor ERXLEBEN, cor-

	In. L.	16th of Lines.	Log.

rected for the heat,　　27　9　3 = 5331　　37268,09

The mean term of
17 obfervations made
upon the firft-floor of
the Crown Inn at
Claufthal, correfpond-
ing for the time to
thofe of Gottingen,　　26　4　8 = 5064　　37044,94

　　　　　　Difference,　　—　　—　　223,15

Mean heat of the air
during the obfervations　−26½　　−29 confeq.
at Gottingen,　　　　　　　　$\frac{58}{1000}$ to deduct.　　12,94
　　At Claufthal,　　−31½

Height of Claufthal above Gottingen,　　　210,22

Le terme moyen entre 17 obfervations du barométre faites à Gottingue, du
24 au 21 Octobre, par M. le Profeffeur ERXLEBEN, corrigées auffi pour la

	P. L.	16 de Lig.	Log.

chaleur,　—　—　—　　27　9　3 = 5331　37268,09

Le terme moyen de 17 obfervations faites au
prémier étage de la Couronne à Claufthal, corre-
fpondantes pour le tems avec celles de Got-　　26　4　8 = 5064　37044,94
tingue,　—　—　—

　　　　Difference,　—　—　—　223,15

Chaleur moyenne de l'air pendant les obfervations
à Gottingue,　—　—　—　−26½　　−29; donc
De même à Claufthal,　—　—　−31½　　$\frac{58}{1000}$ à déduire,　12,94

Hauteur de Claufthal fur Gottingue,　—　—　—　210,21

Mean term of 16 obfervations made at Hanover, from the 24th of October to the 1ft of November, by Mr. DE HINUBER, with a barometer of Mr. DOLLOND's made

	In.	L.	16th of a Line.	Log.
to agree with mine,	28	2	3 = 5411	37332,775
That of the corre- fponding obfervations for the time, made by Mr. ERXLEBEN,	27	9	9 = 5337	37272,972

Difference, — — 59,803

Mean heat at Hano-
ver, at the time of the $\Big\}$ $-29\frac{1}{2}$ $\Big|$ -28, confeq.
obfervations, $\Big|$ $\frac{56}{1000}$ to deduct $\Big\}$ 3,349
The fame at Gottingen, $-26\frac{1}{2}$

Height of Gottingen above Hanover, — 56,454

XXIII. *The general Mathematical Laws which regulate and extend Proportion univerſally; or, a Method of comparing Magnitudes of any Kind together, in all the poſſible Degrees of Increaſe and Decreaſe. By* James Glenie, *A. M. and Lieutenant in the Royal Regiment of Artillery.*

Read March 6, 1777.

THE doctrine of proportion laid down by EUCLID, and the application of it given by him in his Elements, form the baſis of almoſt all the geometrical reaſoning made uſe of by mathematicians both ancient and modern. But the reaſonings of geometers with regard to proportional magnitudes have ſeldom been carried beyond the triplicate *ratio*, which is the proportion that ſimilar ſolids have to one another when referred to their homologous linear dimenſions. This boundary, however, comprehends but a very limited portion of univerſal compariſon, and almoſt vaniſhes into nothing when referred to that endleſs variety of relations, which muſt neceſſarily take place between geometrical magnitudes, in the infinite poſſible degrees of increaſe and decreaſe. The firſt of theſe takes in but

a very

a very contracted field of geometrical comparifon; whereas the laft extends it indefinitely. Within the narrow compafs of the firft, the ancient geometers performed wonders, and their labours have been pufhed ftill farther by the ingenuity and indefatigable induftry of the moderns. But no author, that I have been able to meet with, gives the leaft hint or information with regard to any general method of expreffing geometrically, when any two magnitudes of the fame kind are given, what degree of augmentation or diminution any one of thefe magnitudes muft undergo, in order to have to the other any multiplicate or fub-multiplicate *ratio* of thefe magnitudes in their given ftate; or any fuch *ratio* of them as is denoted by fractions or furds; or (to fpeak ftill more generally) a *ratio* which has, to the *ratio* of the firft-mentioned of thefe magnitudes to the other, the *ratio* of any two magnitudes whatever of the fame but of any kind. Neither have I been able to find that any author has fhewn geometrically in a general way, when any number of *ratios* are to be compounded or decompounded with a given *ratio*, how much either of the magnitudes in the given *ratio* is to be augmented or diminifhed, in order to have to the other a *ratio*, which is equal to the given *ratio*, compounded or decompounded with the other *ratios*. To inveftigate all thefe geome-

trically,

pendicularly to vo, or otherwise if in the fame angle; and let the rectangles or parallelograms MR, NP, be completed. Let LM be a fourth proportional to OP, MN and NR−OP; and let the rectangle or parallelogram LQ be completed.

Then (14. E. 6.) LT is equal to TR, and confequently LQ to MR. But (23. E. 6.) MR has to NP the *ratio* compounded of the *ratios* of MN to NO and NR to OP. Therefore (1. E. 6.) LN has to NO the *ratio* compounded of the *ratios* of MN to NO and NR to OP. But LN is equal to $MN + MN.\frac{NR-OP}{OP}$, or $A + A.\frac{C-D}{D}$, by conftruction. Whence it appears, that a magnitude of the fame kind with A and B, which has to B the *ratio* compounded of the *ratios* of A to B and C to D, is expreffed by $A + A.\frac{C-D}{D}$.

In like manner let E, F, be reprefented by RN, OP, refpectively, and let LK be a fourth proportional to OP, LN, and QR. Then (14. E. 6.) KX is equal to XR or TR and XS together. But fince LN hath already been fhewn to be equal to $A + A.\frac{C-D}{D}$, LK is a fourth proportional to F, E−F, and $A + A.\frac{C-D}{D}$; that is equal to $A.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{E-F}{F}$. by conftruction. Wherefore KN being equal to LK+LN is equal to $A + A.\frac{C-D}{D} + A.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{E-F}{F}$. And fince KQ is equal to LR, KN has to NO a *ratio* compounded of

the

the *ratios* of LN to NO and NR to OP; that is, of the *ratios* A to B, C to D, and E to F. Therefore a magnitude of the same kind with A and B, which has to B the *ratio* compounded of these *ratios* is expressed by $A + A. \frac{C-D}{D} + A. \frac{E-F}{F}$ $+ A. \frac{C-D}{D} . \frac{E-F}{F}.$

Again, if NR, OP, be supposed to represent G, H, respectively, and KV a fourth proportional to OP, KN, and QR; VQ will be equal to KR (14. E. 6.) and consequently VN will have to NO a *ratio* compounded of the *ratios* of KN to NO and NR to OP; that is, of the *ratios* A to B, C to D, E to F, G to H. But VK is by construction equal to $A. \frac{G-H}{H} + A. \frac{C-D}{D} . \frac{G-H}{H} + A. \frac{E-F}{F} . \frac{G-H}{H} + A. \frac{C-D}{D} . \frac{E-F}{F} . \frac{G-H}{H}.$ And this added to KN above found gives $A + A. \frac{C-D}{D}$ $+ A. \frac{E-F}{F} + A. \frac{G-H}{H} + A. \frac{C-D}{D} . \frac{E-F}{F} + A. \frac{C-D}{D} . \frac{G-H}{H} + A. \frac{E-F}{F},$ $\frac{G-H}{H} + A. \frac{C-D}{D} . \frac{E-F}{F} . \frac{G-H}{H},$ for the magnitude of the same kind with A and B, which has to B the *ratio* compounded of the *ratios* A to B, C to D, E to F, G to H; whence the law of continuation is manifest.

The same conclusions may be derived from (E. 5.); so that no principle can be simpler or more geometrical than that here made use of.

Thus then these magnitudes will stand.

I. $A + A. \frac{C-D}{D}$, when two *ratios* are compounded.

2. $A + A \cdot \dfrac{C-D}{D} + A \cdot \dfrac{E-F}{F} + A \cdot \dfrac{C-D}{D} \cdot \dfrac{E-F}{F}$, when three are compounded.

3. $A + A \cdot \dfrac{C-D}{D} + A \cdot \dfrac{E-F}{F} + A \cdot \dfrac{G-H}{H} + A \cdot \dfrac{C-D}{D} \cdot \dfrac{G-F}{F} + A \cdot \dfrac{C-D}{D} \cdot$
$\dfrac{G-H}{H} + A \cdot \dfrac{E-F}{F} \cdot \dfrac{G-H}{H} + A \cdot \dfrac{C-D}{D} \cdot \dfrac{E-F}{F} \cdot \dfrac{G-H}{H}$, when four *ratios* are compounded, &c. &c.

By continuing this operation much farther, I found upon examination that the number of terms in which A is connected with the differences C−D, E−F, G−H, &c. taken one by one, two by two, three by three, &c. if p denote the number of *ratios* compounded, is expreſſed reſpectively by $\dfrac{p-1}{1}$, $\dfrac{p-1}{1} \cdot \dfrac{p-2}{2}$, $\dfrac{p-1}{1} \cdot \dfrac{p-2}{2}$, $\dfrac{p-3}{3}$, &c. Thus if the *ratio* of A to B be ſuppoſed equal to the *ratios* of C to D, E to F, G to H, &c. reſpectively, theſe expreſſions will give the following ones.

1. $A + \dfrac{2-1}{1} \cdot A \cdot \dfrac{A-B}{B}$.

2. $A + \dfrac{3-1}{1} \cdot A \cdot \dfrac{A-B}{B} + \dfrac{3-1}{1} \cdot \dfrac{3-2}{2} \cdot A \cdot \overline{\dfrac{A-B}{B}}^2$.

3. $A + \dfrac{4-1}{1} \cdot A \cdot \dfrac{A-B}{B} + \dfrac{4-1}{1} \cdot \dfrac{4-2}{2} \cdot A \cdot \overline{\dfrac{A-B}{B}}^2 + \dfrac{4-1}{1} \cdot \dfrac{4-2}{2} \cdot \dfrac{4-3}{3}$.

$A \cdot \overline{\dfrac{A-B}{B}}^3$; for magnitudes of the ſame kind with A and B, which have to B reſpectively the duplicate, triplicate, and quadruplicate *ratio* of A to B; where p is ſucceſſively equal to 2, 3, and 4. And univerſally, by the ſame geometrical reaſoning, it is found, that

$A + \dfrac{p-1}{1} \cdot A \cdot \dfrac{A-B}{B} + \dfrac{p-1}{1} \cdot \dfrac{p-2}{2} \cdot A \cdot \overline{\dfrac{A-B}{B}}^2 + $ &c. $A \cdot \overline{\dfrac{A-B}{B}}^{p-1}$ has to B ſuch

B fuch a multiplicate *ratio* of A to B as is expreffed by the number p.

In the reafoning above I fixed on B as the magnitude to which the reft were to be referred; but I might as well have fixed on A or any of the other magnitudes. Thus, for inftance, $B + \frac{p-1}{1}.B.\frac{B-A}{A} + \frac{p-1}{1}.\frac{p-2}{2}.B.\frac{\overline{B-A}^2}{A} + \&c.$ $B.\frac{\overline{B-A}^{p-1}}{A}$ has to A fuch a multiplicate *ratio* of B to A as is expreffed by the number p; or A has to $B + \frac{p-1}{1}.B.\frac{B-A}{A} + \frac{p-1}{1}.\frac{p-2}{2}.B.\frac{\overline{B-A}^2}{A} + \&c.$ $B.\frac{\overline{B-A}^{p-1}}{A}$ the *ratio* of $A + \frac{p-1}{1}.A.\frac{A-B}{B} + \frac{p-1}{1}.\frac{p-2}{2}.A.\frac{\overline{A-B}^2}{B} + \&c.$ $A.\frac{\overline{A-B}^{p-1}}{B}$ to B; that is, fuch a multiplicate *ratio* of A to B as is expreffed by the number p. Each of thefe, indeed, I demonftrated feparately from the fame fort of geometrical reafoning; but for the fake of brevity I omit fetting down thefe feparate demonftrations, as they are both contained in general reafoning above, which furnifhes likewife a great variety of other expreffions, according as certain numbers of the *ratios* C to D, E to F, G to H, &c. are fuppofed to be refpectively equal to, greater or lefs than, the *ratio* of A to B.

XXIV. *The Cafe of* Ann Davenport. *By Mr.* Fielding Beſt Fynney, *Surgeon at* Leek, *in* Staffordſhire; *communicated to* Thomas Percival, *M. D. F. R. S. and by him to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Read April 10, 1777.

I TOOK the liberty, a few days ago, of tranſmitting to you, by a private hand, a ſingular cafe, ſent me by Mr. F. B. FYNNEY, an ingenious ſurgeon, who is ſettled at Leek in Staffordſhire. He deſires that it may be laid before the Royal Society; and I ſhall think myſelf much obliged by your compliance with his requeſt.

TO DR. PERCIVAL.

S I R,

UPON May 16, 1775, being defired to vifit ANN DAVENPORT, a native of this town, I beheld a truly miferable object, with the moft cadaverous countenance I had ever feen, emaciated to the laft degree by a hectic fever, and profufe colliquative fweats. She had a continual thirft, her appetite was totally gone, and fhe was always in the extremes of being too loofe or too bound.

Her mother informed me, that fhe was then in her twenty-firft year; and that fhe had been a ftrong and fprightly child from her birth, until fhe was about five years of age, from which time fhe had been a ftranger to health, and every now and then had been feized with excruciating fits of the colic, efpecially whenever fhe ate or drank any thing the leaft acid.

The young woman told me, that about a year ago fhe had firft perceived a fwelling on the right fide of her belly, juft above the groin; which, if at any time fhe attempted to ftretch out her thigh, gave her inexpreffible pain, as if fomething ftabbed her in that part:

O o o 2

that

that therefore fhe was always obliged to keep up her knees, more or lefs, towards her breaft, by which means fhe had, in fome degree, loft the power of extending her limbs.

I ordered her to take half a drachm of powdered bark in a little red port wine every four hours; and, as matter had already formed within the tumor, I defired that a maturating poultice might be applied every night and morning; for I imagined that nature, without fuch affiftance, could never bring the abfcefs to a head in her weak condition.

July 1oth, the matter pointing at the upper end of the tumour very near the *os ilium*, I made a large opening, from which was difcharged an amazing quantity of *pus*; but, as the tenfion was ftill great, I applied a linfeed poultice over the common dreffings: neverthelefs, in a few days a fecond abfcefs began to form towards the *vertebræ* of the loins, between the falfe ribs and the *os ilium,* which was rapid in its progrefs, for it was brought to maturation, and opened on the 26th.

On the 31ft I was alarmed with a gangrenous appearance of the whole integuments of the *abdomen :* for this fhe took one drachm of powdered bark in red-port every three hours; but, as vefications and every fym-

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ptom

ptom of a *fphacelus* continued to increafe, I likewife ufed the bark externally, in the two following forms, every morning and evening:

℞ *Tinct. Cort. Peruv. Simp.* ʒ ïj
⸻ *Myrrh. Comp.* ʒ j
Sp. Sal. Ammon.
Mell. Ægypt. aa. ʒ ſſ. *m. fiat embrocat.*

℞ *Fæc. Cerevif. acid.* ℔ j
Farin. Avenac. q. s. coque ad confiftentiam Cata-
plafmatis, et adde Pulv. Cort. Peruv. ʒ j.
Ol. Olivar. rec. ʒ iv *m. fiat Cataplafm.*

This treatment foon put the mortification to a ftand, and the parts floughing off largely left three holes, at nearly equal diftances one from another, betwixt the firft opening and the left *os ilium*, befides feveral ones in different parts of the belly; but as the difcharge was immoderate, I looked upon the patient to be in the utmoft danger. However, the fame courfe was perfevered in, and at the latter end of Auguft another abfcefs appeared lower down, towards the right groin; I ordered it to be poulticed, and left it to open of itfelf, which it did on the 21ft of September. I was immediately called to her; and,

and, upon carefully examining the part, I found a hard subſtance deeply ſeated, which I directly extracted [a].

It was making its way towards the integuments from the extremity of the *appendix vermiformis* of the *cæcum*, which probably, and fortunately, by former inflammations had adhered to the *peritonæum*. The large end came firſt, and the ſmall end was within the *appendix vermiformis* of the *cæcum* at the time I took it out; for, immediately upon the extraction, ſome excrements followed, and among them ſome dark brown particles which I diſcovered to be filings of iron, which the patient had formerly taken in a large quantity, as ſhe had never been regular like other women. On a careful examination I found ſome of theſe filings quite reduced to ruſt,

[a] See plate IX. the figures 1. and 2. are different views of the external ſurface of this irregular ſubſtance, and of ſo much of its *nucleus* as projects out of the round part, exactly as both appeared on being taken out of the body. The whole was of a duſky brown colour, and had a great reſemblance to a ſmall ſhrivelled pear. Fig. 3. is a ſection of the round part, which ſeemed to be formed of fine fibrous ſubſtances, cloſely cemented together by an earthy matter, and of the peg of crab-tree wood, its *nucleus*. This figure likewiſe ſhews how far the peg went in, and alſo an incruſtation of ſtony matter upon it.

The *nucleus*, I believe, is the ſmaller end of that part of a ſilk-engine called a ſtar, at which machine the patient had been employed before ſhe was five years of age, therefore it muſt have been lodged at leaſt ſixteen years within the *appendix vermiformis* of the *cæcum*, as ſhe remembers nothing of ſwallowing it, and as during that courſe of years ſhe had frequently been afflicted with the ſevere colics before-mentioned.

but

but ſtill retaining their form as they came from under the file.

Some *fæces* came through this laſt wound daily, frequently moſt copiouſly; and ſometimes (though the external orifice was large) by being confined with the dreſſings, they inſinuated themſelves between the integuments of the *abdomen,* and came through the other openings. About the middle of February 1776, the diſcharge of the excrements by theſe openings was ſenſibly diminiſhed; and the wounds were all healed, except one, by the latter end of the year, through which a ſmall quantity of excrements ſtill continue to paſs now and then.

Her health is, within this ſhort time, ſurprizingly improved; ſhe is now very fleſhy and ſtrong, has had the *catamenia,* and I have the greateſt reaſon to expect that ſhe will be perfectly cured. Strict regard was all along paid to the non-naturals.

As the caſe above is a very uncommon one, I prevailed on the reverend Mr. ROGERS, miniſter of this place, to viſit the patient along with me; and he was ſo fully ſatisfied of the truth of the account, as to give me the following certificate.

I hereby

I hereby certify, that I faw, this 27th day of December, 1775, Mr. FIELDING BEST FYNNEY drefs his patient ANN DAVENPORT, and was an eye-witnefs to pure *fæces* coming out of the wound from which he had extracted an irregular fubftance on the 21ft of September laft.

As witnefs my hand,

J. ROGERS,
Minifter of Leek.

Fig. 1.
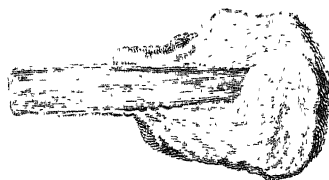
Fig. 3.
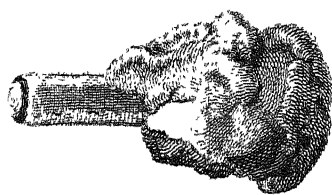
Fig. 2.

XXV. *An Account of the Kingdom of* Thibet. *In a Letter from* John Stewart, *Esquire, F. R. S. to Sir* John Pringle, *Bart. P. R. S.*

S I R,

DURING my late refidence in India, a tranfaction took place in Bengal, which, in its confequence, led to a new and more intimate knowledge of a vaft country, hitherto unexplored by Europeans, and hardly known to them but by name. As every difcovery of this fort tends to the advancement of natural knowledge, I have thought a fhort notice on the fubject might prove no difagreeable communication to the Society; and therefore take the liberty, with your approbation, to fubmit it, in this manner, to them.

The kingdom of Thibet, although known by name ever fince the days of MARCO PAOLO and other travellers, of the twelfth and thirteenth centuries, had never been properly explored by any European till the period of

P p p   which

which I am now to fpeak. It is true, fome ftraggling miffionaries of the begging orders had, at different times, penetrated into different parts of the country; but their obfervations, directed by ignorance and fuperftition, placed in a narrow fphere, could give no ideas but what were falfe and imperfect. Since them, the Jefuits have given the world, in DUHALDE's Hiftory of China, a fhort account of this country, collected, with their ufual pains and judgement, from Tartar relations, which, as far as it goes, feems to be pretty juft.

This country commonly paffes in Bengal under the name of Boutan. It lies to the northward of Hindoftan, and is all along feparated from it by a range of high and fteep mountains, properly a continuation of the great Caucafus, which ftretches from the ancient Media and the fhores of the Cafpian fea, round the north-eaft frontiers of Perfia, to Candahar and Caffamire, and thence, continuing its courfe more eafterly, forms the great northern barrier to the various provinces of the Mogol empire, and ends, as we have reafon to believe, in Affam or China. This ftupendous Tartar bulwark had ever been held impaffable by the Mogols, and all other Muffulman conquerors of India: and although in the vallies lying between the lower mountains, which run out perpendicular to the main ridge, there refide various Indian people,

whom

whom they had occafionally made tributary to their
power, they never had attempted a folid or permanent
dominion over them. It was on occafion of a difputed
fucceffion between the heirs of one of the Rajah's or petty
fovereigns of thofe people, that the Boutaners were called
down from their mountains to the affiftance of one of
the parties; and our government engaged on the oppofite
fide. The party affifted by us did not fail in the end to
prevail; and in the courfe of this little war two people
became acquainted who, although near neighbours, were
equally ftrangers to each other. At the attack of a town
called Cooch Behar, our troops and the Boutaners firft
met; and nothing could exceed their mutual furprize in
the rencounter. The Boutaners, who had never met in
the plains any other than the timid Hindoos flying naked
before them, faw, for the firft time, a body of men, uni-
formly cloathed and accoutred, moving in regular order,
and led on by men of complexion, drefs, and features,
fuch as they had never beheld before: and then the ma-
nagement of the artillery, and inceffant fire of the muf-
quetry, was beyond any idea which they could have con-
ceived of it. On the other hand, our people found them-
felves on a fudden engaged with a race of men unlike
all their former opponents in India, uncouth in their ap-
pearance, and fierce in their affault, wrapped up in furs,

and armed with bows and arrows and other weapons peculiar to them.

The place was carried by our troops, and a great many things taken in the fpoil, fuch as arms, cloathing, and utenfils of various forts. Images in clay, in gold, in filver, and in enamel, were fent down to Calcutta; all which appeared perfectly Tartar, as we have them reprefented in the relations and drawings of travellers; and there were befides feveral pieces of Chinefe paintings and manufactures. Whilft thofe things continued to be the fubject of much converfation and curiofity to us in Bengal, the fame of our exploits in the war had reached the court of Thibet, and awakened the attention of the Tayfhoo Lama, who (the Delai Lama being a minor) was then at the head of the ftate. The Dah Terriah, or Deb Rajah as he is called in Bengal (who rules immediately over the Boutaners, and had engaged them in the war) being a feudatory of Thibet, the Lama thought it proper to interpofe his good offices, and in confequence fent a perfon of rank to Bengal, with a letter and prefents to the governor, to folicit a peace for the Dah, as his vaffal and dependant.

Mr. HASTINGS, the governor, did not hefitate a moment to grant a peace at the mediation of the Lama, on the moft moderate and equitable terms; and, eager to

<div align="right">feize</div>

feize every opportunity which could promote the interest and glory of this nation, and tend to the advancement of natural knowledge, propofed in council to fend a perfon in a public character to the court of the Tayfhoo Lama, to negotiate a treaty of commerce between the two nations, and to explore a country and people hitherto fo little known to Europeans. Mr. BOGLE, an approved fervant of the company, whofe abilities and temper rendered him every way qualified for fo hazardous and uncommon a miffion, was pitched on for it. It would be foreign to my purpofe to enter into a detail of his progrefs and fuccefs in this bufinefs: it will be fufficient to fay, that he penetrated, acrofs many difficulties, to the center of Thibet; refided feveral months at the court of the Tayfhoo Lama; and returned to Calcutta, after an abfence of fifteen months on the whole, having executed his commiffion to the entire fatisfaction of the adminiftration. I have reafon to believe that Mr. BOGLE will one day give to the world a relation of his journey thither, accompanied with obfervations on the natural and political ftate of the country. I only, in the mean time, beg leave to mention a few particulars, fuch as my recollection of his letters and papers enable me to give.

Mr. BOGLE divides the territories of the Delai Lama into two different parts. That which lyes immediately

contiguous

contiguous to Bengal, and which is called by the inhabitants Docpo, he diftinguifhes by the name of Boutan; and the other, which extends to the northward as far as the frontiers of Tartary, called by the natives Pû, he ftyles Thibet. Boutan is ruled by the Dah Terriah or Deb Rajah, as I have already remarked. It is a country of fteep and inacceffible mountains, whofe fummits are crowned with eternal fnow; they are interfected with deep vallies, through which pour numberlefs torrents that increafe in their courfe, and at laft, gaining the plains, lofe themfelves in the great rivers of Bengal. Thefe mountains are covered down their fides with forefts of ftately trees of various forts; fome (fuch as pines, &c) which are known in Europe; others, fuch as are peculiar to the country and climate. The vallies and fides of the hills which admit of cultivation are not unfruitful, but produce crops of wheat, barley, and rice. The inhabitants are a ftout and warlike people, of a copper complexion, in fize rather above the middle European ftature, hafty and quarrelfome in ther temper, and addicted to the ufe of fpirituous liquors; but honeft in their dealings, robbery by violence being almoft unknown among them. The chief city is Taffey Seddein fituated on the Patchoo. Thibet begins properly from the top of the great ridge of the Caucafus, and extends from thence

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in breadth to the confines of Great Tartary, and perhaps to fome of the dominions of the Ruffian empire. Mr. BOGLE fays, that having once attained the fummit of the Boutan mountains, you do not defcend in an equal proportion on the fide of Thibet; but, continuing ftill on a very elevated bafe, you traverfe vallies which are wider and not fo deep as the former, and mountains that are neither fo fteep, nor apparently fo high. On the other hand, he reprefents it as the moft bare and defolate country he ever faw. The woods, which every where cover the mountains in Boutan, are here totally unknown; and, except a few ftraggling trees near the villages, nothing of the fort to be feen. The climate is extremely fevere and rude. At Chamnànning, where he wintered, although it be in latitude $31° 39'$, only 8° to the northward of Calcutta, he often found the thermometer in his room at 29° under the freezing point by FAHRENHEIT's fcale; and in the middle of April the ftanding waters were all frozen, and heavy fhowers of fnow perpetually fell. This, no doubt, muft be owing to the great elevation of the country, and to the vaft frozen fpace over which the north wind blows uninterruptedly from the pole, through the vaft defarts of Siberia and Tartary, till it is ftopped by this formidable wall.

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The Thibetians are of a fmaller fize than their fouthern neighbours, and of a lefs robuft make. Their complexions are alfo fairer, and many of them have even a ruddinefs in their countenances unknown in the other climates of the eaft. Thofe whom I faw at Calcutta appeared to have quite the Tartar face. They are of a mild and chearful temper; and Mr. BOGLE fays, that the higher ranks are polite and entertaining in converfation, in which they never mix either ftrained compliments or flattery. The common people, both in Boutan and Thibet, are cloathed in coarfe woollen ftuffs of their own manufacture, lined with fuch fkins as they can procure; but the better orders of men are dreffed in European cloth, or China filk, lined with the fineft Siberian furs. The ambaffador from the Deb Rajah, in his fummer-drefs at Calcutta, appeared exactly like the figures we fee in the Chinefe paintings, with the conical hat, the tunick of brocaded filk, and light boots. The Thebetian who brought the firft letter from the Lama was wrapped up from head to foot in furs. The ufe of linen is totally unknown among them. The chief food of the inhabitants is the milk of their cattle, prepared into cheefe, butter, or mixed with the flour of a coarfe barley or of peas, the only grain which their foil produces; and even thefe articles are in a fcanty proportion: but they are furnifhed

with

with rice and wheat from Bengal and other countries in their neighbourhood. They alſo are ſupplied with fiſh from the rivers in their own and the neighbouring provinces, ſalted and ſent into the interior parts. They have no want of animal food from the cattle, ſheep, and hogs, which are raiſed on their hills; and are not deſtitute of game, though I believe it is not abundant. They have a ſingular method of preparing their mutton, by expoſing the carcaſe entire, after the bowels are taken out, to the Sun and bleak northern winds which blow in the months of Auguſt and September, without froſt, and ſo dry up the juices and parch the ſkin, that the meat will keep uncorrupted for the year round. This they generally eat raw, without any other preparation. Mr. BOGLE was often regaled with this diſh, which, however unpalatable at firſt, he ſays, he afterwards preferred to their dreſſed mutton juſt killed, which was generally lean, tough, and rank. It was alſo very common for the head men, in the villages through which he paſſed, to make him preſents of ſheep ſo prepared, ſet before him on their legs as if they had been alive, which at firſt had a very odd appearance.

The religion and political conſtitution of this country, which are intimately blended together, would make a conſiderable chapter in its hiſtory. It ſuffices for me to

fay, that at prefent, and ever fince the expulfion of the Eluth Tartars, the kingdom of Thibet is regarded as depending on the empire of China, which they call Cathay; and there actually refide two mandarines, with a garrifon of a thoufand Chinefe, at Lahaffa the capital, to fupport the government; but their power does not extend far: and in fact the Lama, whofe empire is founded on the fureft grounds, perfonal affection and religious reverence, governs every thing internally with unbounded authority. Every body knows that the Delai Lama is the great object of adoration for the various tribes of heathen Tartars, who roam through the vaft tract of continent which ftretches from the banks of the Volga to Correa on the fea of Japan, the moft extenfive religious dominion, perhaps, on the face of the globe. He is not only the fovereign Pontiff, the vicegerent of the Deity on earth; but, as fuperftition is ever the ftrongeft where it is moft removed from its object, the more remote Tartars abfolutely regard him as the Deity himfelf. They believe him immortal, and endowed with all knowledge and virtue. Every year they come up from different parts, to worfhip and make rich offerings at his fhrine; even the emperor of China, who is a Mantchou Tartar, does not fail in acknowledgements to him in his religious capacity, and actually entertains at a great

expence,

expence, in the palace of Pekin, an inferior Lama, deputed as his Nuncio from Thibet. It is even reported, that many of the Tartar chiefs receive certain prefents, confifting of fmall portions of that, from him, which is ever regarded in all other perfons as the moft humiliating proof of human nature, and of being fubject to its laws, and treafure it up with great reverence in gold boxes, to be mixed occafionally in their ragouts. It is, however, but juftice to declare, that Mr. BOGLE ftrenuoufly infifts, that the Lama never makes fuch prefents; but that he often diftributes little balls of confecrated flour, like the *pain benit* of the Roman catholics, which the fuperftition and blind credulitity of his Tartar votaries may afterwards convert into what they pleafe. The orthodox opinion is, that when the grand Lama feems to die, either of old age or of infirmity, his foul in fact only quits an actual crazy habitation to look for another younger or better, and it is difcovered again in the body of fome child, by certain tokens known only to the Lamas or Priefts, in which order he always appears. The prefent Delai Lama is an infant, and was difcovered only a few years ago by the Tayfhoo Lama, who in authority and fanctity of character is next to him, and confequently, during the other's minority, acts as chief. The Lamas, who form the moft numerous as well as the moft

powerful

powerful body in the ftate, have the priefthood entirely in their hands; and, befides, fill up many monaftic orders which are held in great veneration among them. Celibacy, I believe, is not pofitively enjoined to the Lamas; but it is held indifpenfable for both men and women, who embrace a religious life: and indeed their celibacy, their living in communities, their cloyfters, their fervice in the choirs, their ftrings of beads, their fafts, and their penances, give them fo much the air of Chriftian monks, that it is not furprizing an illiterate capuchin fhould be ready to hail them brothers, and think he can trace the features of St. Francis in every thing about them. It is an old notion, that the religion of Thibet is a corrupted Chriftianity; and even Father DISEDERII, a Jefuit (but not of the Chinefe miffion) who vifited the country about the beginning of this century, thinks he can refolve all their myfteries into ours; and afferts, with a true myftical penetration, that they have certainly a good notion of the Trinity, fince, in their addrefs to the Deity, they fay as often Konciok-oik in the plural as Konciok in the fingular, and with their rofaries pronounce thefe words, Om, ha, hum. The truth is, that the religion of Thibet, from whence-ever it fprung, is pure and fimple in its fource, conveying very exalted notions of the Deity, with no contemptible fyftem of morality; but in its

<div align="right">progrefs</div>

progrefs it has been greatly altered and corrupted by the inventions of worldly men, a fate we can hardly regret in a fyftem of error, fince we know that that of truth has been fubject to the fame. Polygamy, at leaft in the fenfe we commonly receive the word, is not in practice among them; but it exifts in a manner ftill more repugnant to European ideas; I mean in the plurality of hufbands, which is firmly eftablifhed and highly refpected there. In a country where the means of fubfifting a family are not eafily found, it feems not impolitic to allow a fet of brothers to agree in raifing one, which is to be maintained by their joint efforts. In fhort, it is ufual in Thibet for the brothers in the family to have a wife in common, and they generally live in great harmony and comfort with her; not but fometimes little diffenfions will arife (as may happen in families conftituted upon different principles) an inftance of which Mr. BOGLE mentions in the cafe of a modeft and virtuous lady, the wife of half a dozen of the Tayfhoo Lama's nephews, who complained to the uncle, that the two youngeft of her hufbands did not furnifh that fhare of love and benevolence to the common ftock which duty and religion required of them. In fhort, however ftrange this cuftom may appear to us, it is an undoubted fact that it prevails in Thibet in the manner I have defcribed.

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The manner of beſtowing their dead is alſo ſingular: they neither put them in the ground like the Europeans, nor burn them like the Hindoos; but expoſe them on the bleak pinnacle of ſome neighbouring mountain, to be devoured by wild beaſts and birds of prey, or waſted away by time and the viciſſitudes of weather in which they lie. The mangled carcaſes and bleached bones lye ſcattered about; and, amidſt this ſcene of horror, ſome miſerable old wretch, man or woman, loſt to all feelings but thoſe of ſuperſtition, generally ſets up an abode, to perform the diſmal office of receiving the bodies, aſſigning each a place, and gathering up the remains when too widely diſperſed.

The religion of Thibet, although it be in many of its principal *dogmata* totally repugnant to that of the Bramins or of India, yet in others it has a great affinity to it. They have, for inſtance, a great veneration for the cow; but they transfer it wholly from the common ſpecies to that which bears the tails, of which I ſhall ſpeak hereafter. They alſo highly reſpect the waters of the Ganges, the ſource of which they believe to be in Heaven; and one of the firſt effects which the treaty with the Lama produced, was an application to the governor-general, for leave to build a place of worſhip on its banks. This it may be imagined was not refuſed; and,

when

when I left Bengal, a fpot of ground was actually af-
figned for that purpofe, about two or three miles from
Calcutta. On the other hand, the Sunniaffes, or Indian
pilgrims, often vifit Thibet as a holy place, and the Lama
always entertains a body of two or three hundred in his
pay. The refidence of the Delai Lama is at Pateli, a vaft
palace on a mountain near the banks of the Barampoo-
ter, about feven miles from Lahaffa. The Tayfhoo
Lama has feveral palaces or caftles, in one of which Mr.
BOGLE lived with him five months. He reprefents the
Lama as one of the moft amiable as well as intelligent
men he ever knew; maintaining his rank with the ut-
moft mildnefs of authority, and living in the greateft pu-
rity of manners, without ftarchnefs or affectation. Every
thing within the gates breathed peace, order, and digni-
fied elegance. The caftle is of ftone or brick, with many
courts, lofty halls, terraces, and porticos; and the apart-
ments are in general roomy, and highly finifhed in the
Chinefe ftile, with gilding, painting, and varnifh. There
are two conveniencies to which they are utter ftrangers,
ftair-cafes and windows. There is no accefs to the upper
rooms but by a fort of ladders of wood or iron; and for
windows they have only holes in the cielings, with pent-
houfe covers, contrived fo as to fhut up on the weather-
fide. Firing is fo fcarce, that little is ufed but for culinary
purpofes;

purpofes; and they truft altogether for warmth in their houfes to their furs and other cloathing. The Lama, who is compleatly converfant in what regards Tartary, China, and all the kingdoms in the Eaft, was exceedingly inquifitive about Europe, its politics, laws, arts and fciences, government, commerce, and military ftrength; on all which heads Mr. BOGLE endeavoured to fatisfy him, and actually compiled for his fervice a brief ftate of Europe in the Hindoftan language, which he ordered to be tranflated into that of Thibet. The Lama being born at Latack, a frontier province next Caffamire, is fully mafter of the Hindoftan language, and always converfed with Mr. BOGLE in it; but the people, who are perfuaded he underftands all languages, believed he fpoke to him in Englifh, or, as they call it, the European tongue. The Ruffian Empire was the only one in Europe known to him: he has a high idea of its riches and ftrength, and had heard of its wars and fuccefs againft the Empire of Rome (for fo they call the Turkifh ftate); but could not conceive it could be in any wife a match for Cathay. Many of the Tartar fubjects of Ruffia come to Thibet; and the Czar has even, at various times, fent letters and prefents to the Lama. Mr. BOGLE faw many European articles in his hands; pictures, looking-glaffes, and trinkets of gold, filver, and fteel, chiefly Englifh, which

he

he had received that way, particularly a GRAHAM's repeating watch, which had been dead, as they said, for some time. While he was there, several Mongols and Calmucs arrived from Siberia, with whom he conversed.

The city of Lahaffa, which is the capital, is of no inconfiderable fize, and is reprefented as populous and flourifhing. It is the refidence of the chief officers of government, and of the Chinefe mandarins and their fuite. It is alfo inhabited by Chinefe and Caffemirian merchants and artificers, and is the daily refort of numberlefs traders from all quarters, who come in occafional parties, or in ftated caravans. The waters of the Great River, as it is emphatically called in their language, wafh its walls. Father DUHALDE, with great accuracy, traces this river, which he never fufpects to be the Barampooter, from its origin in the Caffemirian mountains (probably from the fame fpring which gives rife to the Ganges) through the great valley of Thibet, till, turning fuddenly to the fouthward, he lofes it in the kingdom of Affam; but ftill, with great judgement and probability of conjecture, fuppofes it reaches the Indian fea fomewhere in Pegu or Aracan. The truth is, however, that it turns fuddenly again in the middle of Affam, and, traverfing that country wefterly, enters Bengal towards Rangamatty, under the above-mentioned name, and thence

bending its courfe more foutherly, joins the Ganges, its fifter and rival, with an equal, if not more copious ftream; forming at the conflux a body of running frefh water, hardly to be paralleled in the known world, which difembogues itfelf into the Bay of Bengal. Two fuch rivers uniting in this happy country, with all the beauty, fertility, and convenience which they bring, well entitles it to the name of the Paradife of Nations, always beftowed upon it by the Moguls.

The chief trade from Lahaffa to Pekin is carried on by caravans that employ full two years in the journey thither and back again; which is not furprizing, when we confider that the diftance cannot be lefs than two thoufand Englifh miles; and yet it is to be obferved, that an exprefs from Lahaffa reaches Pekin in three weeks, a circumftance much to the honour of the Chinefe police, which knows to eftablifh fo fpeedy and effectual a communication through mountains and defarts for fo long a way. The trade with Siberia is carried on by caravans to Seling, which is undoubtedly the Selialinfky of the Ruffian travellers on the borders of Baykal lake. And this accounts for an extraordinary fact mentioned by BELL; that, on the banks of the river of that name, he one day found a man bufy in redeeming, from fome boys who were angling, the fifh they caught, and throwing them into

the

the water again; and from this circumftance, and the mark on his forehead, knew him to be an Indian. On converfing with him, he found his conjecture to be right. The man told him, he came from Madrafs, had been two years on his journey, and mentioned by name fome of the principal Englifh gentlemen there. This Indian, no doubt, muft have travelled as a Faquier or Sunniaffy through Bengal into Thibet, and from thence paffed with the caravan to Selonginfky, where BELL found him. It is proper to remark, that the Indians have an admirable method of turning godlinefs into great gain, it being ufual for the Faquiers to carry with them, in their pilgrimages from the fea-coafts to the interior parts, pearls, corals, fpices, and other precious articles, of fmall bulk, which they exchange on their return for gold-duft, mufk, and other things of a fimilar nature, concealing them eafily in their hair and in the cloths round their middle, and carrying on, confidering their numbers, no inconfiderable traffic by thefe means. The Goffeigns are alfo of a religious order, but in dignity above the Faquiers; and they drive a more extenfive and a more open trade with that country.

A particular account of the commerce would be foreign to the purport of this letter; but, as it would leave the information which I wifh to convey very incompleat,

pleat, did I not mention the fources from which this country, fo apparently poor and unfruitful, draws a fupply of the foreign articles of convenience and luxury, which I have occafionally faid they poffefs; I fhall juft obferve, that, befides their lefs traffic with their neighbours in horfes, hogs, rock-falt, coarfe cloths, and other articles, they enjoy four ftaple articles, which are fufficient in themfelves to procure every foreign commodity of which they ftand in need; all of which are natural productions, and deferve to be particularly noticed. The firft, though the leaft confiderable, is that of the cowtails, fo famous all over India, Perfia, and the other kingdoms of the Eaft. It is produced by a fpecies of cow or bullock different from what I believe is found in any other country. It is of a larger fize than the common Thibet breed, has fhort horns, and no hump on its back. Its fkin is covered with whitifh hair of a filky appearance; but its chief fingularity is in its tail, which fpreads out broad and long, with flowing hairs, like that of a beautiful mare, but much finer and far more gloffy. Mr. BOGLE fent down two of this breed to Mr. HASTINGS, but they died before they reached Calcutta. The tails fell very high, and are ufed, mounted on filver handles, for Chrowras, or brufhes, to chafe away the flies; and no man of confequence in India ever goes out, or fits in

form

form at home, without two Chowrawbadars, or brufhers, attending him, with fuch inftruments in their hands.

The next article is the wool from which the Shaul, the moft delicate woollen manufacture in the world, fo much prized in the Eaft, and now fo well known in England, is made. Till Mr. BOGLE's journey our notions on that fubject were very crude and imperfect. As the Shauls all come from Caffemire, we concluded the material from which they were fabricated to be alfo of that country's growth. It was faid to be the hair of a particular goat, the fine under hair from a camel's breaft, and a thoufand other fancies; but we now know it for certain to be the produce of a Thibet fheep. Mr. HASTINGS had one or two of thefe in his paddock when I left Bengal. They are of a fmall breed, in figure nothing differing from our fheep, except in their tails, which are very broad; but their fleeces, for the finenefs, length, and beauty of the wool, exceed all others in the world. The Caffemirians engrofs this article, and have factors eftablifhed for its purchafe in every part of Thibet, from whence it is fent to Caffemire, where it is worked up, and becomes a fource of great wealth to that country, as well as it is originally to Thibet.

Mufk is another of their ftaple articles, of which it will be needlefs to fay much, as the nature, quality, and

value,

value of this precious commodity are fo well known in Europe. I fhall only remark, that the deer which produces it is common in the mountains; but being excefsively fhy, and frequenting folely the places the moft wild and difficult of accefs, it becomes a trade of great trouble and danger to hunt after. We have the mufk fent down to Calcutta in the natural bag, not without great rifk of its being adulterated; but ftill it is far fuperior to any thing of the kind that is to be met with in fale in Europe.

The laft of the articles which I reckon ftaple is gold, of which great quantities are exported from Thibet. It is found in the fands of the Great River, as well as in moft of the fmall brooks and torrents that pour from the mountains. The quantity gathered in this manner, though confiderable with refpect to national gain, pays the individual but very moderately for the labour beftowed on it. But, befides this, there are mines of that metal in the northern parts, which are the referved property of the Lama, and rented out to thofe who work them. It is not found in ore, but always in a pure metallic ftate (as I believe it to be the cafe in all other mines of this metal) and only requires to be feparated from the fpar, ftone, or flint, to which it adheres. Mr. HASTINGS had a lump fent to him at Calcutta, of about the fize of

-a bullock's

a bullock's kidney, which was a hard flint veined with folid gold. He caufed it to be fawed in two, and it was found throughout interlarded (if I may be allowed the expreffion) with the pureft metal. Although they have this gold in great plenty in Thibet, they do not employ it in coin, of which their government never ftrikes any; but it is ftill ufed as a medium of commerce, and goods are rated there by the purfe of gold-duft, as here by money. The Chinefe draw it from them to a great amount every year, in return for the produce of their labour and arts.

I could wifh to add to this account fomething refpecting the plants and other botanical productions of this country; but I would not prefume to offer any thing but what is authentic and exact, as far as my knowledge goes. Mr. BOGLE will no doubt be able to fatisfy the learned in that branch, refpecting many things of which I have at prefent no information. He fent down to Calcutta many feeds, grains, kernels, and fruits, part of which only arrived fafe. Of the laft I tafted feveral, they were chiefly of the European forts, fuch as peaches, apples, pears, &c. and therefore more defirable for us in Bengal; but they were all to me infipid and bad.

I am now, SIR, to clofe thefe remarks with craving your forgivenefs for having thus ftarted a new fubject of

6   curiofity,

curiofity, without the means of giving more compleat light concerning it. Time and opportunity may put more in my power on my return to India. In the mean time, I hope the Society will accept as a rarity the tranflation of the original letter which the Tayfhoo Lama wrote to Mr. HASTINGS, by the envoy whom he fent to folicit a peace for the Deb Rajah. It came into my hands in the courfe of my office, and by the permiffion of the Governor general I retained a copy.

The original is in Perfian, a language which the Lama was obliged to employ, that of Thibet, although very elegant and expreffive, as it is faid, being totally unintelligible in Bengal. A letter under the fanction of a character fo long talked of in the weftern world, but fo little known, alone renders it an object of curiofity; but, when it is found to contain fentiments of juftice, benevolence, and piety, couched in a fimple ftyle, not without dignity, and in general exempt from the high-flown compliments and ftrained metaphors fo common among the other people of the Eaft, I have no doubt of its being received with approbation; at any rate, it will ferve as a fpecimen of the way of thinking and writing among a people whofe country and manners I have made the fubject of the foregoing fketch.

Tranflation

Tranflation of a Letter from the Tayfhoo Lama *to* Mr. HASTINGS, *Governor of* Bengal, *received the* 29*th of* March, 1774.

THE affairs of this quarter in every refpect flourifh: I am night and day employed for the increafe of your happinefs and profperity. Having been informed, by travellers from your quarter, of your exalted fame and reputation, my heart, like the bloffom of fpring, abounds with fatisfaction, gladnefs, and joy. Praife God that the ftar of your fortune is in its afcenfion. Praife him, that happinefs and eafe are the furrounding attendants of myfelf and family. Neither to moleft or perfecute is my aim: it is even the characteriftic of our fect to deprive ourfelves of the neceffary refrefhment of fleep, fhould an injury be done to a fingle individual; but in juftice and humanity, I am informed you far furpafs us. May you ever adorn the feat of juftice and power, that mankind may, in the fhadow of your bofom, enjoy the bleffings of peace and affluence! By your favour I am the Rajah and Lama of this country, and rule over a

number

number of fubjects; a particular with which you have no doubt been acquainted by travellers from thefe parts. I have been repeatedly informed, that you have been engaged in hoftilities againft the Dah Terria, to which it is faid the Dah's own criminal conduct, in committing ravages and other outrages on your frontiers, gave rife. As he is of a rude and ignorant race, paft times are not deftitute of the like mifconduct which his avarice tempted him to commit. It is not unlikely but he has now renewed thofe inftances, and the ravages and plun-der which he may have committed on the fkirts of the Bengal and Bahar provinces, have given you provocation to fend your vindictive army againft him. However, his party has been defeated, many of his people have been killed, three forts have been taken from him, and he has met with the punifhment he deferved. It is as evident as the Sun that your army has been victorious; and that, if you had been defirous of it, you might in the fpace of two days have entirely extirpated him, for he had not power to refift your efforts. But I now take upon me to be his mediator; and to reprefent to you, that, as the faid Dah Terria is dependant upon the Dalai Lama, who rules in this country with unlimited fway (but, on account of his being in his minority, the charge of the govern-ment

ment and adminiſtration for the preſent is committed to me) ſhould you perſiſt in offering further moleſtation to the Dah's country, it will irritate both the Lama and all his ſubjects againſt you. Therefore, from a regard to our religion and cuſtoms, I requeſt you will ceaſe all hoſtilities againſt him; and in doing this you will confer the greateſt favour and friendſhip upon me. I have reprimanded the Dah for his paſt conduct; and I have admoniſhed him to deſiſt from his evil practices in future, and to be ſubmiſſive to you in all things. I am perſuaded he will conform to the advice which I have given him; and it will be neceſſary that you treat him with compaſſion and clemency. As to my part, I am but a Faquier[a]; and it is the cuſtom of my ſect, with the roſary in our hands, to pray for the welfare of mankind, and for the peace and happineſs of the inhabitants of this country; and I do now, with my head uncovered, intreat that you may ceaſe all hoſtilities againſt the Dah in future. It would be needleſs to add to the length of this letter, as the bearer of it, who is a Goſeign[b], will repreſent to

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[a] The original being in Perſian, this word is uſed, which can only be applied with propriety to a perſon of the Muſſulman faith: here it can only mean a religious perſon in general. Perhaps monk would have been the beſt tranſlation.

[b] This means a religious perſon of the Hindoo ſect.

you all particulars; and it is hoped you will comply there-with.. In this country, worſhip of the Almighty is the profeſſion of all. We poor creatures are in nothing equal to you; having, however, a few things in hand, I ſend them to you by way of remembrance, and hope for your acceptance of them.

XXVI. *Of the Degrees and Quantities of Winds requisite to move the heavier Kinds of Wind Machines. In a Letter from* John Stedman, *M. D. Fellow of the Royal College of Physicians at Edinburgh, to the Reverend* Samuel Horsley, *LL.D. Secretary to the Royal Society.*

S I R,

Read Apr. 24, 1776.

THE irregularity and uncertainty of winds in this country have been found a confiderable difcouragement to erect wind machines. It hath frequently happened, that the proprietors of coal and other works, after having reared thefe kinds of engines, and having found them infufficient for the intended work, have been obliged to open mines, or to erect fire machines. This is chiefly owing to the undertakers reckoning upon more winds of a fufficient power to move thefe machines, than we commonly have in this country.

Thefe machines are rarely erected with us, unlefs where a confiderable moving power is neceffary. This is always the cafe where the larger kind of pump-work is to be kept in motion, or where water is to be extracted

from

from deep pits.   Having enquired of many people con-
cerned in fuch works, what may be the proportion of
time in which wind machines may be kept in motion, to
that in which they cannot move from a defect of wind,
I found thefe people differing widely in their conjectures.

Having, however, met with one gentleman of ob-
fervation and accuracy, who had erected a wind ma-
chine for draining his coal; he told me that, by the beft
computation he had been able to make, he never could
depend upon more than fifty-three or fifty-four hours of
wind fufficient for moving that machine, in a week,
taking the year round.   This is below what is commonly
believed to be the proportion; but, fo far as this can be
rated by an eftimate in the following manner, it will be
found to be much about what may be depended upon
for the heavieft kinds of machines; ftill making allow-
ance for the differences of expofures, and for the ftrength
and frequency of winds in one part of the country more
than in another.   We may here take notice of a circum-
ftance favourable to the draining of coal-pits; that is,
that the periods of the year in which the greateft quan-
tities of rain fall, are likewife obferved to abound with
winds of the higher degrees.   This feldom fails to hold
in hilly countries, and particularly in thofe of high lati-
tudes; that is, where the differences of heats in fummer

and

and of colds in winter are very confiderable. The periods of the year here meant will generally be found to fall near the Equinoxes.

The only method of bringing the matter to a proper eftimate is, by comparing the quantities of winds fufficient for moving thefe machines, with thofe of winds below that degree, and calms. This computation can only be drawn from journals in which the degrees of winds are noted. In the meteorological regifter of the Medical Effays of the Edinburgh Society, there is a column of winds, and four degrees are noted. This divifion is fufficient for the purpofes for which that regifter was intended; but, when we confider the wind as a power acting upon machines, that number of degrees will be found too fmall.

Thus, from the fecond in that regifter to a hurricane, there is but one intermediate degree. As the fecond degree, which is very moderate, is infufficient for moving thefe machines; the third is more than juft enough for that purpofe. A degree, therefore, which is a mean betwixt thefe two, will be found to be the loweft that is fufficient for moving machines of the heavier kinds, particularly fuch as are ufed for pumping water out of coal-pits.

Thefe

Thefe three degrees of wind, that is the fecond and third of the Edinburgh regifter, and an intermediate degree, are very diftinguifhable even by the fenfes, and without the affiftance of any inftrument, by thofe who are attentive and have been accuftomed to make obfervations of this nature.

To afcertain proportions of this nature, a longer term of years would have been more fatisfactory; but, in cafe others fhould afterwards purfue this kind of computation, the proportions are digefted in two tables at the end of this effay, and may be confulted occafionally.

In making up thefe tables, *viz.* one of the fecond degree and above, and the other of the third and upwards, hurricanes are included, though that degree of wind be too high for any machine. But, as the obfervations were taken twice in twenty-four hours, and as winds fufficient to move thefe machines may be fuppofed, to have happened fometimes between the times of obfervation, though at thefe times the wind might have been below the mean; to compenfate this defect, hurricanes are included in the computation.

From thefe tables then we have the following proportions of the two degrees of winds and upwards, to thofe below; and likewife of the mean betwixt thofe two degrees.

Days.

Winds of the second degree and upwards in
each week, — — } 4.283

Calms and winds below the second degree, 2.717

Winds of the third degree and upwards, 0.902

Calms and winds below the third degree, 6.098

Winds of a mean proportion between the two
preceding degrees, — — } 2.592

Calms and winds below the mean degree, 4.408

The proportion of these winds in the year comes out in weeks and fractional parts thus:

Weeks.

Winds of the second degree and upwards in
the year, — — — } 31.903

Calms and winds below the second degree, 20.239

Winds of the third degree and upwards, 6.719

Calms and winds below the third degree, 45.423

Winds of a mean proportion between the two
preceding, — — — } 19.307

Calms and winds below the mean, — 32.835

From this computation we have 2.592 days in a week, or 19,307 weeks in a year, in which wind machines of

the heavier kinds, and of confiderable friction, may be fuppofed to be kept in motion; which, to the times wherein they cannot go, is as 10 to 17.

It may be obferved, that the refiftance to the machine, or its weight and friction, being diminifhed, though in a fmall degree, will add confiderably to the frequency and length of times in which it can go; fince it often happens that there are winds immediately below the loweft degree in the preceding eftimate, fufficient to keep the lighter machines in motion.   Hence thofe who have machines which are not abfolutely of the heavieft kind, will be apt to conclude this computation erroneous.   Befides, there are few who make allowance for, or attend to, the univerfal law which obtains in mechanics, that in larger machines, their power doth not increafe in a proportion fo high as their bulk and the refiftance arifing from their friction.

Computations of this nature, if carried on for a fufficient length of time, might be of fome ufe in regulating infurances, or in pointing out the rifks of nautical adventurers, when made in the fame climates with the calculation of winds.

Here I fhould have concluded; but having, after writing what is above, committed thefe tables and obfervations to the examination of a learned member of your So-

ciety,

ciety, much converfant in thefe matters, I had the fatif-
faction to find that he thought them worthy to be
communicated to the Royal Society; but remarked, that
the materials, which I had proceeded upon, were not fo
applicable to the purpofe as could have been wifhed.
He thinks, that the degrees of winds, being only dif-
tinguifhed into four in the journals from whence thofe
tables have been compiled, are much too few to take in
thofe of the weaker kind, that will however turn well-
conftructed wind-mills. Indeed I regretted that the table
from which I made my eftimate contained fo few de-
grees; but it was for that reafon I calculated an inter-
mediate degree between the fecond and the third of our
meteorological regifter. Now as all the degrees above
that intermediate degree are fufficient to move the hea-
vieft machine, and the degrees below it infufficient for
that purpofe, fo far as I have been able to obferve, it
comes to be the fame as if the table, from which I made
the eftimate, had confifted of eight degrees, fuppofing a
mean proportion to be found between the other degrees:
thus, 0, $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, &c. this laft number $2\frac{1}{2}$, being
the loweft degree which I find fufficient for the heavieft
machines, would have been 5, had thefe fractional parts
been integers in the table, fo that the higheft degree, in-
ftead of 4 would have been 8. The mean, therefore,

between

between 2 and 3 being found, will, if I miſtake not, anſwer the preceding objection.

This worthy member, at the end of his obſervations, ſays, a mill ſo conſtructed may be expected to go the half of the year; that is, I preſume, a wind-mill conſtructed in the neateſt and moſt ingenious manner. But this, I have reaſon to believe, is far from being the caſe with wind-mills in this country, they being for the moſt part clumſy. I doubt not but wind-mills, the conſtruction of which this ingenious gentleman hath directed, though of the ſame ſize and conſiſting of the ſame numbers with thoſe I have ſeen here, will neverthelefs be moved by a lower degree of wind, and conſequently will go a greater proportion of time, though they have the ſame re-ſiſtance to overcome as others leſs artificially conſtructed. Indeed the ſame wind machine, as is well known, will require a degree of wind conſiderably higher when its joints are dry or become gummy, than when they are ſufficiently greaſed. In my eſtimate I have all along had an eye to the wind machines which have the greateſt reſiſtance to overcome, and conſequently the machines themſelves of the largeſt kind. But when the learned gentleman ſuppoſes a machine to go one half of the year, he may perhaps not underſtand one abſolutely of

the.

the largeft kind. Now as the powers of fimilar ma-
chines, but of different magnitudes, are as their cubes,
but the refiftance arifing from their weight and friction
as the fourth power; it follows, that a fmall difference in
the fize of two or more wind machines will require con-
fiderable differences in the winds neceffary for their
movements.

I am, &c.

TABLE I.

Shewing the Proportion of winds of the second degree and upwards, to those of the first and below, for five years.

	Proportion of winds of the 2d degree to those of the first, &c. in 1731 and 1732.		Proportion, &c. in 1732 and 1733.		Proportion, &c. in 1733 and 1734.		Proportion, &c. in 1734 and 1735.		Proportion, &c. in 1735 and 1736.		Sum of each month for five years.	
	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.
June	16	44	46	14	26	34	24	36	41	19	153	147
July	7	55	44	18	39	23	29	33	43	19	162	148
August	8	54	27	35	20	42	28	34	47	15	130	180
September	30	30	24	36	40	20	34	26	34	26	162	138
October	26	36	13	49	36	26	31	31	35	27	141	169
November	39	21	18	42	51	9	16	44	33	25	157	141
December	43	19	29	33	38	24	46	16	41	21	197	113
January	55	7	43	19	35	27	58	4	45	17	236	74
February	45	13	46	10	48	8	47	9	42	16	228	56
March	39	23	50	12	40	22	53	9	46	16	228	82
April	44	16	34	26	42	18	46	14	46	14	212	88
May	54	8	28	34	36	26	55	7	57	5	230	80
Sum of years and months,	406	326	402	328	451	279	467	263	510	220	2236	1416

TABLE II.

Shewing the proportion of winds of the third degree and upwards, to those of the second and below, for five years.

	Proportion, of winds of the 3d degree to those of the first, &c. in 1731 and 1732.		Proportion, &c. i. 1732 and 1733.		Proportion, &c. in 1733 and 1734.		Proportion, &c. in 1734 and 1735.		Proportion, &c. in 1735 and 1736.		Sum of each month for five years.	
	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.
June	1	59	—	60	3	57	2	58	1	59	7	233
July	1	61	7	55	11	51	5	57	1	61	25	285
August	—	62	—	62	8	54	3	59	—	62	48	252
September	13	47	9	51	10	50	8	52	8	52	11	299
October	9	53	1	61	12	50	11	51	2	60	35	275
November	11	49	8	52	19	41	2	58	6	54	46	254
December	17	45	4	58	12	50	9	53	4	58	46	264
January	5	57	11	51	9	53	18	44	6	56	49	261
February	24	34	20	36	24	32	12	44	7	51	87	197
March	12	50	10	52	18	44	6	56	5	57	51	259
April	9	51	5	55	9	51	8	52	5	55	36	264
May	7	55	—	62	9	53	10	52	4	58	30	280
Sum of years and months,	109	623	75	655	144	586	94	636	49	683	471	3183

XXVII. *De—*

XXVII. *Description of the Jesuits Bark Tree of* Jamaica *and the* Caribbees. *By* William Wright, *M. D. Member of the Philosophical Society of* America, *and Surgeon-general in* Jamaica. *Communicated by* Joseph Banks, *Esquire, F. R. S.*

Read April 24, 1777.

THIS species of Jesuits bark grows on stony lands near the sea-shore, in the parishes of St. James and Hanover, on the north-side of Jamaica; and I found one small tree, at a little distance from the fort, at Martha Brae in the parish of Trelawny. The tree is called the Sea-side Beech, and rises only to twenty feet. The trunk is not thick in proportion, but hard, tough, and of a yellowish-white colour in the inside. The branches and leaves are opposite; the leaves are of a rusty green, and the young buds of a blueish green hue. It blossoms in November, and continues in flower till February, having on the same tree or sprig, flowers and ripe pods. The flowers are of a duskish yellow colour, and the pods black: when ripe they split in two, and are, with their flat brown seeds, in every respect similar to

those

thofe of the *Cinchona officinalis* as depicted in a plate fent out by Mr. BANKS.

The bark of this tree in general is fmooth and grey on the outfide, though in fome rough and fcabrous. When well dried, the infide is of a dark-brown colour. Its flavour at firft is fweet, with a mixture of the tafte of horfe-radifh and of aromatics of the Eaft; but, when fwallowed, of that very bitternefs and aftringency which characterifes the Peruvian bark. It yields thefe qualities ftrongly to water both when cold and in decoction. Half an ounce, boiled from two pounds to one pound of water, made as ftrong a decoction as three times its weight of the *Cinchona vera*. The colour was brown, but not turbid.

I have had many opportunities of trying its effects, efpecially in remittents, which are the moft common and fatal fevers in thefe climes. A vomit or gentle purge, if neceffary, was firft given; and then immediately this bark fo foon as they operated. I obferved that it ftrengthened the ftomach, checked retching and vomiting, corrected morbid humours in *primæ viæ*, and conquered fpeedily the difeafe. My fuccefs in fuch a dangerous malady leaves not a doubt on my mind, but that it will prove equally efficacious in every other cafe where a tonic and antifeptic medicine is indicated.

CINCHONA JAMAICENCIS, feu CARIBBEANA.

CINCHONA CARIBÆA, *Linn. Spec. Plant.* 245.

FOL. ovata, integerrima, acuta, enervia, oppofita.

FLOR. fingulares, axillares.

CAL. *Perianthium* monophyllum, fuperum, quinque-fidum, minimum, perfiftens, campanulatum, obfo-letiffimè quinquedentatum.

COR. monopetala, infundibiliformis. *Tubus* cylindraceus, longiffimus: *Limbus* quinquepartitus, tubo equa-lis: *Laciniis* ovatis, oblongis, reflexis, quandoque pendulis.

STAM. *Filamenta* quinque, filiformia, erecta e medio tubi, longitudine corollæ. *Antheræ* longiffimæ, obtufæ, erectæ fupra bafin exteriorem, affixæ in fauce corollæ.

CAPS. bipartibilis in duas partes diffepimento paralello, latere inferiore dehifcens.

SEM. plurima, compreffa, marginata, oblonga.

Cinchona Jamaicensis

XXVIII. *Defcription and Ufe of the Cabbage-bark Tree of* Jamaica. *By* William Wright, *M. D.; communicated by* Richard Brocklefby, *M. D. F. R. S.*

Read May 1, 1777. THE Cabbage-bark tree, or Worm-bark tree, grows in moft parts of Jamaica, and particularly abounds in the low Savannahs of St. Mary and St. George. It rifes to a confiderable height, but no great thicknefs, fending off branches towards the top of a ftraight, fmooth trunk. The leaves are, when young, of a light-green hue; when full-grown, of a dark-green colour; and before they drop, of a rufty appearance.

The flower-fpike is long and beautifully branched. The flowers are numerous; their *calyces* of a dark purple; their *petals* of the colour of the pale-rofe; the *nectaria* muft contain much honey, as thoufands of bees, beetles of various kinds, butter-flies, and humming-birds, are continually feeding thereon.

The *pericarpium* is a green, hard fruit, of the fize of the fmaller plumb. The fkin is of the thicknefs of a crown-piece; and taftes very auftere. The kernel is covered with a brown fkin like that of other nuts; it is very hard, and taftes aftringent.

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The

The wood is hard, and takes a good polifh. It is however fit only for rafters or other parts of fmall buildings; but this tree is valued chiefly for its bark, which externally is of a grey colour, and the infide black and furrowed.

Frefh cabbage-bark taftes mucilaginous, fweet, and infipid. Its fmell, however, is rather difagreeable, and it retains it in the decoction; hence by fome called the bulge-water tree.

Mr. PETER DUGUID, formerly of this ifland, feems to have been the firft that gave any account of the virtues of this bark, in the Edinburgh Effays, Phyfical and Literary, vol. II. The experiments he promifed have never yet appeared. It is certain it has powerful effects, and its anthelmintic quality is eftablifhed by the experience of feveral ages. It is at prefent in general ufe here, and begins to be known in Europe. No defcription having yet appeared, I have fupplied that defect as far as my abilities in Botany reached. It remains now to proceed to its exhibition, and the purpofes it is meant to anfwer as a medicine.

Cabbage-bark may be given in different forms; as in decoction, fyrup, powder, and extract. I have ufed them all, and fhall fpeak of them feparately.

The

The decoction.   Take fresh-dried or well-preserved cabbage-bark, one ounce. Boil it in a quart of water, over a flow fire, till the water is of an amber colour, or rather of deep coloured Madeira wine; strain it off, sweeten it with sugar, and let it be used immediately, as it does not keep many days.

Syrup of Cabbage-bark.   To any quantity of the above decoction add a double portion of sugar, and make a syrup. This will retain its virtues for years.

The extract of cabbage-bark is made by evaporating the strong decoction in *balneo mariæ* to the proper confistence; it must be continually stirred, as otherwise the resinous part rises to the top, and on this probably its efficacy depends.

The powder of well-dried bark is easily made, and looks like jallap, though not of equal specific gravity.

This bark, like most other powerful anthelmintics, has a narcotic effect; and on this account it is always proper to begin with small doses, which may be gradually increased till a nausea is excited, when the dose for that patient is ascertained. But by frequent use we can in common determine the dose, though we chuse to err rather on the safe side.

A strong healthy grown person may, at first, take four table spoonfuls of the decoction or syrup, three grains of the extract, or thirty grains of the powder for a dose.

A youth,

A youth, three table fpoonfuls of the decoction or fy-rup, two grains of extract, or twenty grains of powder.

A perfon of ten years of age, two table fpoonfuls of the decoction or fyrup, one grain and a half of extract, or fifteen grains of the powder.

Children of two or three years old, a table fpoonful of the decoction or fyrup, one grain of extract, or ten grains of the powder. Children of a year old, half the quantity.

Thefe may be increafed, as above obferved, till a naufea is excited, which will depend on the ftrength, fex, and habit of body of the patient.

Care muft be taken that cold water be not drank during the operation of this medicine, as it is in this cafe apt to occafion ficknefs, vomiting, fever, and delirium. When this happens, or when an over large dofe has been given, the ftomach muft be wafhed with warm water: the patient muft fpeedily be purged with Caftor-oil and ufe plenty of lime-juice beverage for common drink; vegetable acid being a powerful antidote in this cafe, as well as in an over dofe of opium.

The decoction is what is moftly given here, and feldom fails to perform every thing that can be expected from an anthelmintic medicine, by deftroying worms in the inteftines, and bringing them away in great quantities. By frequent ufe, however, thefe animals become fami-

liarized, and we find it neceſſary to intermit it, or have recourſe to others of inferior merit.

The writers of the Edinburgh Medical Commentaries take notice, that the decoction of cabbage-bark always excites vomiting. We find no ſuch effect from it here, and may account for it by their receiving it in a mouldy ſtate. A ſyrup, therefore, is given there with better effect. They obſerve alſo that it has a diuretic virtue, which we have not taken notice of here.

This bark purges pretty briſkly, eſpecially in powder, thirty or forty grains working as well as jallap by ſtool; but in this way it does not ſeem to kill worms ſo well as in decoction.

Five grains of the extract made a ſtrong man ſick, and purged him ſeveral times; but, by frequent uſe, he took ten grains to produce at length the ſame effect.

It muſt not be concealed that fatal accidents have happened from the imprudent adminiſtration of this bark, chiefly from over-doſing the medicine. But this cannot detract from the merit of the cabbage-bark, ſince the beſt medicines, when abuſed, become deleterious; and even our beſt aliments, in too great quantity, prove deſtructive. Upon the whole, the cabbage-bark is a moſt valuable remedy, and I hope will become an addition to the *materia medica*.

U u u 4　　　　　GEOFFRÆA

GEOFFRÆA JAMAICENCIS INERMIS.

FOL. oppofita, oblongo-ovata, ternata, acuminata, fu-
pernè glabra, infernè enervia, petiolis brevibus.

CAL. *Perianthium* monophyllum, campanulatum, le-
viffimè quinquepartitum, *laciniis* ovatis, brevibus.

COR. papilionacea: *Vexillum* fubrotundum, concavum:
Alæ obtufæ, concavæ, longitudine vexilli. *Carina*
ovata, patens, in duabus partibus leviffimè divifa.

STAM. diadelpha, decem, filiformia, in calyce inferta,
longitudine alarum. *Antheræ* fubrotundæ.

PIST. fubulatum, filiforme. *Stigma* nullum. *Germen*
ovato-oblongum, compreffum.

PER. *Drupa* fub-ovata, magna.

SEM. *Nux* fub-ovata, fub-lignea, fulco utrinque longi-
tudinali, bivalvis.

The botanical reader will fee how nearly this agrees
with the *Geoffræa fpinofa* of LINNÆUS. The Genera of
plants are fufficiently multiplied, and it was thought beft
to make this a fpecies only.

Geoffroea jamaicensis inermis

made exprefsly in London and Paris, in order to make ſuch experiments as might preſent themſelves to me *en courrant*; and which, either from want of acquaintance with the ſubject, want of time, or want of money, become rarely the object of travellers; but remain wholly unknown till princely munificence and philoſophic zeal (of which we have a recent inſtance) unite in producing them to the world. After the very celebrated and ingenious labours of Mr. DE LUC, farther inveſtigation of the ſubject of barometrical meaſurement might ſeem unneceſſary, if not invidious; but, furniſhed as I was with an apparatus every way ſufficient for the inquiry, finding myſelf in the country which had been the ſcene of his operations, and poſſeſſing ſome ſhare of his own zeal, I could not but gratify the curioſity I had to verify and repeat his experiments: if therefore in the purſuit of this inquiry I ſhould be led to a concluſion ſomething different from the reſult of his own obſervations, I am convinced that this diſtinguiſhed obſerver, of whoſe candour and talents I have an equal opinion, will impute it wholly to a love for truth; as with me the precept applies as ſtrongly to the philoſopher as to the hiſtorian, *Ne quid falſi audeat, ne quid veri non audeat dicere.*

But

But to proceed. The inftruments I made ufe of in thefe operations were, two of RAMSDEN's barometers [a]; three or four thermometers detached from the barometers, whofe boiling and freezing points I had examined myfelf; an equatorial inftrument, the circles of which were about feven inches diameter, made by RAMSDEN; a fifty-feet fteel meafuring chain; and three three-feet rods, two of deal and one of brafs, in order to examine and correct the chain, thefe latter made by BARADELLE at Paris. Befides thefe I took with me a little bell-tent, which I found of great ufe, as it defended me from the wind and fun; and I may remark, that the obfervations of the uppermoft barometer were made in the tent.

My firft feries of obfervations I propofed to be on Mont Saleve[b], one of the Alps, fituated about two

(a) It may not be improper to remark, that the fpecific gravity of the quick-filver of thefe barometers with 68° of heat was 13,61; the diameter of the bore of the tube 0,20 inch; and that of the refervoir 1,5 inch.

(b) Mont Saleve extends near nine miles in length; is not quite 3300 feet in height above the Lake. That fide of it which is next Geneva is for the moft part a barren rock, the north-eaft end of it being almoft a perpendicular precipice; the other fide of the mountain is lefs rude, of a more gentle accli-vity, covered with trees, fhrubs, and herbage, as is alfo the top, where is fome of the fineft pafture in the world. It is inhabited only by a few fhepherds, who pafs the fummer months here with their cattle, in little miferable huts or barns: the remaining part of the year, *viz.* for about four or five months, it is covered with fnow. This mountain contains chiefly a calcareous ftone; and there is reafon to believe that there is an iron ore in it, at leaft in fome parts of it, as a piece Mr. DE LUC, the brother, picked up near the fouth-weft end, I found, fenfibly affected the magnet.

leagues

leagues ſouth of Geneva, and preciſely on the ſame point where Mr. DE LUC had made his higheſt or fifteenth ſtation: this ſpot I learnt from his brother, whoſe civilities, both then and ſince, I ſhall frequently have occaſion to remember and mention.

The place where I meaſured my baſe was in a field near the villages of Archamp and Neidens, not quite three miles in a horizontal line from the top of the rock whoſe height was to be determined (ſee the chart that accompanies this account). At the end of the baſe A I intended to place one of my barometers; and the other at the top of the rock, called the Pitton, at c; and with the above inſtruments meaſure the triangle ABC. The angles were taken both on the horary circle, which was brought parallel to the horizon, and alſo on the azimuth circle of the equatorial inſtrument; this made it, as it were, two different inſtruments independant of each other. The angles were moreover doubled, tripled, and quadrupled, on each arch; by this means the error of the center or axis of the inſtrument vaniſhed; the poſſible error in the diviſions, in the reading off, and in the coincidence of the wires in the teleſcope (which magnified forty times) with the ſignals placed at each angle of the triangle, was leſſened in proportion to the number of times the obſervation was repeated; and finally the

mean

mean of all was taken. The fame was done with each angle at A, B, and C, horizontal as well as vertical, *viz.* the elevation of C above A and B was taken; and alfo the depreffion of A and B below C. The advantage of this method was, that the error of the line of collimation, the effect of refraction, and of the curvature of the earth's furface, all became equal and contrary; by thefe means the little errors were diminifhed, and great errors abfolutely avoided[c]. I fhall, however, beg leave to fet down the operation at length refpecting this one triangle, in order to fhew the precifion that may be expected from fuch a geometrical procefs; to remove the fcruples of thofe gentlemen who fufpect that accuracy is only to be obtained by large quadrants; and laftly, to do juftice and fatisfaction to the celebrated artift who invented and made this valuable inftrument.

(*c*) I muft acknowledge here, that the attraction of the mountain creeps into the account uncorrected for, but only half of this quantity influences the mean refult, as at the top it was nothing, and at the bottom of the mountain it could not exceed 10″ in the direction AC, as I find from a rough computation, the half of which = 5″ would give only four inches for the correction.

Determination of the Baſe.

	Ch.	Ft.	In.	Temper.
Length of the baſe AB (ſee the Chart) by the chain, firſt time, — — — }	55	10	0	71°
Ditto, ſecond time, — — —	55	9	9¾	76
The mean, — —	55	9	10.87	73½

	Ft.	In.	
By frequent previous obſervations I determined *(d)* the length of the chain by compariſon with the braſs ſtandard rod reduced to 60° of heat, — }	50	0	0	60
Correction for 13½° of heat from expanſion, —	+0	0.	05	
Diameter of the pins or arrows, one of which was uſed at each chain, and in ſuch manner, that this correction became always + — — — }	+0	0	16	
Correct length of the chain as it was uſed in meaſuring the baſe, — — — — }	50	0	21	
Multiply by the number of entire chains in the baſe, —			55	
	2750	11	55	
Add the parts of a chain, — —	+ 9	10	87	
True length of the baſe, as it was meaſured, —	2760	10	42	
Correction for the defect of level, taken with an inſtrument made on purpoſe, each time the chain was placed, }	— 0	76 *(e)*		
The true horizontal diſtance between A and B becomes,	2760	9	66	

Deter-

(*d*) It may be required, to what preciſion I could determine the length of my chain? I think certainly to within $\frac{3}{100}$ of an inch, or $\frac{1}{2000}$ of the whole length. The common GUNTER's chain of the ſhops is always ſubject to ſpring and ſtretch conſiderably; mine was made of hardened ſteel, on purpoſe to avoid this defect. It however ſtill preſerved ſome degree of elaſticity, for when pulled with a force of about ten pounds, it ſeemed $= 0,12$ inch longer than when laid gently on the floor without being ſtretched at all: the aſſumed length of the chain was ſuch as ſeemed to me probable from a moderate tenſion in

common

Determination of the angles by the equatorial.

			On the azimuth circle.	On the equat. circle, the horary being converted into gradual divifions.
			° ′ ″	° ′ ″
∠ A by the 1ft obfervation	—		58 27 30	—	58 28 30
2d,	—	—	— 29 0	—	— 27 30
3d,	—	—	— 28 30	—	— 29 15
4th,	—	—	— 30 15	—	— 29 15
∠ taken four times over on the arch,	—		233 54 15	—	233 54 30
The mean,	—		58 28 49	—	58 28 37½

Laftly, the mean of all from the two circles
= $58° 28' 43\frac{1}{4}''$ = ∠ at A.

∠ B by the 1ft obfervation,	—		111 54 45	—	111 53 0
2d,	—	—	— 51 30	—	— 52 30
3d,	—	—	— 50 30	—	— 50 45
∠ taken three times over on the arch,	—		335 36 45	—	335 36 15
Mean,	—	—	111 52 15	—	111 52 5

Mean of all from the two circles = $111° 52' 10''$
= ∠ at B.

common ufing it. It may perhaps not be out of place to remark here, that the rods with which the chain was examined, agreed exactly with the fcales of the barometers; at leaft the difference in nine inches, taken in different parts of the fcale, did not appear to exceed $\frac{1}{1000}$ of an inch.

(e) The precaution in taking the inclination of the chain every time, if the bafe be nearly a plain, as is the cafe in many meadows, feems to be unneceffary; for this fame correction, deduced from the inclination of the bafe obferved at A and B, comes out —0,99 inch, only 0,23 inch different, a quantity wholly inconfiderable.

∠ a

			On the azimuth circle.	On the equat. circle.
			o ′ ″	o ′ ″
∠ c by the 1ft obfervation,	—		9 39 0	9 38 30
2d,	—	—	— 39 0	— 38 15
3d,	—	—	— 38 45	— 39 45
∠ taken four times over on the arch,	—		38 35 45	38 34 45
Mean,	—	—	9 38 56¼	9 38 41¼

Mean of the two circles, $= 9° \ 38′ \ 48\frac{3}{4}″ = \angle$ at c.

			By actual obfervation.		Angles finally corrected.
			o ′ ″		o ′ ″
∠ at A,	—	—	58 28 43¼	These angles corrected by adding 6″ to each (the fum of their errors, or defect from 180° being —18″) become,	58 28 49¼
∠ at E,	—	—	111 52 10		111 52 16
∠ at C,	—	—	9 38 48¾		9 38 54¾
Sum of the three angles =			179 59 42	Sum,	180 0 0
Taken from			180 0 0		
Leaves the difference = } fum of the errors,			— 18		

It is highly curious and fatisfactory to fee the amazing correfpondency of thefe obfervations, made with an inftrument of only $3\frac{1}{2}$ inches radius, whereon an angle of one minute is about equal $\frac{1}{1300}$ inch; and I think we may fairly conclude, that the corrected mean refult of thefe obfervations is true to within $6″$ or $8″$[(f)]; which, as

(f) I may have a future occafion to fpeak of the accuracy of this inftrument for aftronomical purpofes; but I cannot omit this opportunity of mentioning one, *viz.* in taking the latitude of the city of Amiens in Picardy, where I had thirteen obfervations by the ftars and Sun, the mean of which differed 25″ from the extremes, and only 3″ from the refult of Mr. CASSINI's obfervations, made, I believe, with a nine-feet zenith fector, as related in *La Meridienne de Paris verifiée.* —5

may

may be proved hereafter, would occafion an error of only three feet in the diftance of the mountains, and feven inches in the height. I proceed next to the vertical angles.

Determination of the inclination of the fides AC, BC, *and*
   AB, *with the horizon*; *the height of the eye at the inftrument being four feet above the ground.*

	Altitude from below at A.			Depreffion from above at C.		
	°	′	″		°	′	″
Inclination of AC, —	10	33	2				
Correction for the part of the fignal which was obferved, } —		1	38	Correct for the fignal, — —— for the line of collimation, — }		+	16
						+	59
Correction for the line of collimation, — }		0	59	—— for refraction,		+	27
Correct for the refraction, —		0	27	True depreffion of A from C,	10	31	0
				Arch intercepted between, or curvature, — }		—	2	30
True Altitude of c from A,	10	29	58				
				True altitude of c from A deduced from the obfervation at c, }	10	28	30

Mean corrected altitude of c from A = 10° 29′ 14″ (g).

(g) If the computation were to be made from either of the obfervations taken feparately, the difference would amount to only three feet in the height of c; and this may either be in the correction of the line of collimation, the effect of refraction, or in miftaking the part of the fignal that was obferved; for, whilft I was gone to the top of the mountain, fome peafants poffeffed themfelves of the handkerchiefs I had fixed to the fignals below in order to have a confpicuous and determined point.

Altitude from below at z.					Depreſſion from above at c.		
	o	′	″		o	′	″
Inclination of BC, —	11	20	26		11	19	47
Correct for the part of the ⎱ signal obſerved, — ⎰		— 1	38	Correct for the ſignal, —	—	—	59
				Error of collimation, —	—	+	59
Error of collimation,	—	0	59	Effect of refraction, —	—	+	26
Correct for refraction,	—	0	26				
				True depreſſion of B from c,	11	20	18
True altitude of c from B,	11	17	23	Arch intercepted, or cur-⎱ vature, — ⎰	—	2	18
				True altitude of c from B, ⎱ deduced from the ob- ⎬ ſervation at c, — ⎰	11	18	0

Mean of the two, or corrected altitude of c from B
$= 11° 17' 41\frac{1}{2}''$.

Altitude at A.				Depreſſion at B.		
	o	′	″		o	′	″
∠ of inclination of AB ⎱ the baſe, — ⎰	0	27	0		0	27	4
				Error of collimation,	+	0	59
Error of the line of col- ⎱ limation, — ⎰	—	0	59	Correct depreſſion of A ⎱ from B, — ⎰	0	28	3
Correct altitude of B from A,	0	26	1	Arch intercepted, —	—	0	27
				Altitude of B from A de-⎱ duced from the obſer- ⎬ vation at B, — ⎰	0	27	36

Mean of the two, or corrected altitude of B from A
$= 0° 26' 49''$[b].

(b) It ſhould ſeem from theſe two obſervations, that the error of the line of collimation had been aſſumed too great; it has however, as I have before obſerved, nothing to do with the mean reſult: and this is, perhaps, one of the beſt means of diſcovering the error of collimation, and the very method Mr. DE LUC uſed, to adjuſt his level, though, as I have been informed by his brother, without taking into the account the effect of curvature, which, if his horizontal marks were 2000 feet diſtant from each other, would amount to 20″, and the error to half that quantity.

I have

I have thus, in a manner rather prolix, given a detail of the methods ufed to afcertain the quantity of the different angles. It may be of ufe on a like occafion, and will at leaft ferve to determine within what limits the error of the final refult may be expected to lye, as on the precifion of the geometrical operations all the comparifons of the barometrical ones depend. This procefs once mentioned will exempt me and the reader from the trouble a fecond time, when he is informed, that the fame fidelity and pains were employed (where the circumftances would admit) in all the trigonometrical obfervations, of which the annexed chart is a fummary. I proceed now to the determination of the fides, the computations of which are too well known to enter into this paper.

	Feet.
Side AB	2760.8
AC	15286.4
BC	14041.7

Feet.

Thefe with the angles give for the height of c above A, — 2835.07

The height of c above B, — — — 2806.27

The height of B above A, — — — 22.18

Thefe two added give the height of c above A, deduced from the obfervation at B, — — — } 2828.45

But the height by actual obfervation at A was, — — 2835.07

Then the mean of the two, — — — 2831.76

which is probably within three or four feet of the truth, or about one foot in a thoufand.

Having

Having thus the perpendicular height, as I think, very accurately afcertained, it remained for me to take the altitude of the barometer at each ftation A and C, and if poffible with equal precifion. Thefe obfervations it would be too tedious to fet down at length. I fhall, however, premife the following particulars. Every obfervation of the barometer was triple; that is, the height was read off three different times, and the mean taken; but from once reading only I could be fure of the height to $\frac{2}{1000}$ of an inch, exclufive of the error of the divifions, which in fome places might amount to that quantity; this the nonius would itfelf difcover and even correct by eftimation. At every feries of obfervations the float at the bottom was readjufted, fo that I could conftantly be fure of an alteration of the weight of the atmofphere exprefled by 0.002 inch of quickfilver, if not of half that quantity. Finally, the difference of the two barometers [1] was conftantly taken, after being left three-quarters

ters

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[1] It may be concluded, that this difference fhould be conftant, and always the fame; but, from what caufe I know not, it did not appear fo to me. In my journal for the weather for 1775, I find the following note: from a mean of feventeen obfervations between Auguft 12th and Sept. 1. *viz.* before, at, and after, my expedition to Mont Saleve and the Mole, 1 find the difference between my two barometers =,0042 inch, N° 1. ftanding the higheft; in thefe comparifons, however, the extremes fometimes differed from the mean =,006. And in my paffage over Mont Cenis, Dec. 1. barometer N° 1. ftood lower than N° 2.

ters of an hour or more in the fame place, to acquire the true temperature of the air, and this before and after every expedition. The fractional parts of a degree on both the attached and detached thermometers were noted only by eftimation, but written down to 10ths, being more convenient in the computation; for I may remark, that one-third of a degree on the attached thermometer is equal to about $\frac{1}{1000}$ inch on the barometer; this attention, therefore, to the fub-divifions of the degrees became neceffary. I conclude, laftly, with prefuming, that the weight [k] of any column of air may be meafured with thefe barometers to ,008 inch, though all the errors fhould lye the fame way.

Leaving Geneva about half paft fix in the morning, Auguft 20th, I arrived at the place A of my bafe a little before eight; near to which there happened to be a fhepherd's houfe, in which I left one of my barometers (N° 1.) with a fervant, to examine and obferve it every five or ten minutes for near nine hours fucceffively,

by ,013 inch: it is difficult to account for this. May 10th, 1776, at Rome, N° 1. ftood loweft by ,001. June 12th, at Naples, N° 1. ftood loweft by ,008. Sept. 10th, in London, N° 1. ftood higheft by +,006. Thefe apparent variations may poffibly arife from fome alteration in the frame-work of the barometers through moifture, &c.

(k) I muft not be underftood to mean, that the length of any column of air may be meafured to an equal accuracy, even though our theory fhould be perfect: this will be the fubject of inquiry in its proper place.

until

until I returned; the windows and doors of the room, in which the inftrument was placed, being left open, by which means there was a free communication with the outward air, and the barometer not expofed to the Sun. The detached thermometer was hung on the window towards the north-eaft, where there was neither direct nor reflected heat from the Sun [1]. The two barometers

(1) I have thought proper to mention this, as it is almoft the only circum-ftance wherein my method of obferving differed from Mr. DE LUC's, whofe thermometers (if I miftake not) were hung always in the Sun, and probably for this reafon, becaufe the column of the atmofphere between the two baro-meters, whofe mean heat is to be determined, is (if the Snn fhine) all expofed to the Sun. I have, however, always preferred hanging them in the fhade, and I give the following reafons: all fpurious and local heat from reflection is more eafily avoided; no concentrated and falfe heat is acquired by the mounting, and thence communicated to the tube, even though the ball fhould be infulated; and, finally, becaufe I fufpect the real temperature of the atmofphere in the Sun and in the fhade to be the fame, or at leaft infenfibly different. This may be thought to be advancing too much; but, to be fatisfied of the pofition, I made no lefs than four-fcore obfervations with four different thermometers of very different mounting, hung alternately expofed to the Sun's rays, and fcreened from them by the fhade of a tree, in an open plain at fome diftance from the town of Geneva. The refult was, that my beft thermometer, with the ball infulated, differed only 2° in the different fituations; the others, more or lefs, as they were more or lefs connected with the frames in which they hung. One of them, inclofed in a glafs tube, rofe 12° higher than the true temperature, which was 77°. It fhould feem then, that the variety in the mounting occa-fioned this difference; and this effect of the materials, of which the inftru-ment is made, cannot be wholly avoided, as the glafs itfelf, which conftitutes the ball of the thermometer, will acquire and contain more or lefs, in proportion to its thicknefs and opacity. If a thermometer were perfect, it would reflect all the rays that it receives. More might be added to corroborate this idea, but it would fwell this note to an unwarrantable length.

were

were here compared; and at a quarter after nine, begin-
ning my walk, I arrived, not without fome fatigue, at
the top of the mountain about noon. The view from
thence was incredibly beautiful. Every object, that from
Geneva was ftriking, from thence appeared with an ad-
ditional effect. The mountains feemed higher and
nearer; the plain appeared a more perfect level, the fmall
inequalities from this height becoming infenfible; and
a larger portion of the lake prefented itfelf: behind me
an innumerable collection of naked points and precipices,
all new objects, that from below are hid by the mountain,
afforded frefh and moft aftonifhing ideas of this very
fingular part of the creation. The clouds however (for
it was a little hazy) unfortunately prevented my feeing
Mont Blanc and the Glacieres, which were ftill farther
behind. Some of the clouds were below me, and very
near; exhibiting to me, at that time, a very fingular phe-
nomenon of the thunder grumbling under my feet. I
was occupied here between four and five hours with dif-
ferent obfervations. The barometrical ones I am now
going to relate; and I fhall at the fame time give the
computations of them according to Mr. DE LUC's me-
thod, or rather according to Dr. HORSLEY's reduction of
it to the fcales and meafures of this country (*vide*
Philof. Tranf. vol. LXIV.) with this difference, that I

have

have reckoned the equation for the expanfion of quick-
filver =,00323 inch for every degree of FAHRENHEIT's
thermometer in a column of 30 inches, inftead of ,00312
which Mr. DE LUC ufed; the former I had collected from
fome of my own experiments made at Oxford in the
beginning of the year 1773: this difference will not,
however, occafion an alteration in the refult of any one
of my obfervations of more than five inches, and may
therefore be confidered as of no account. Of the real
value of this correction I fhall fpeak more hereafter.

The barometer was fet up on the mountain at one
o'clock, and left an hour and a quarter to acquire the
temperature of the tent in which it was placed, before
the firft regular feries of obfervation was taken. The
fucceeding obfervations were made at intervals of near
an hour each. I have ventured to fet down the height
of the barometer to ,0001 inch; but this is only the
mean from three or four readings off. It feems that the
heat of the tent was confiderably greater than that of the
external air; this, however, can only influence the ex-
panfion of the quickfilver, fhewn by the attached ther-
mometer, and not the preffure of the atmofphere.
Laftly, the true difference in the height of the refervoirs
of the two barometers, by comparifon with A and c, was
found equal 2831.3 feet geometrically.

Comparison of the first series.

Observations at the top of the mountain at c.

	Barom. N° 2. above at c. In. Pts.	Therm. attached.	Therm. detached.
	25.7120	78.0	65.0
Correct for the diff. of the 2 attached therm. 5°.9,	— 162		
Barometer at the top,	25.6958	Log.	4098621
————— below,	28.3951	Log.	4532434

Difference, or fall of the quickfilver, } 2.6993 Diff. of Log. 433.813 { the height in English fathoms,

Correct for 29°.7 of heat, — — + 28.728

Correct height in fathom, — — 462.541

× 6

Height in English feet by the barometer, — 2775.246
Height by the trig. method, — — 2831.3

Difference, or error of the barometer $\frac{198}{10000}$, — 56.1

Observations below at A.

	Barom. N° 1. below at A. In. Pts.	Therm. attached.	Therm. detached.
	28.3990	72.1	73.9
Correct for the diff. of the barometer,	— 39		65.0 heat at c.
	28.3951		69.4 mean heat of the air.
			39.7 { stand. temp. according to Dr. HORSLEY.

+ 29.7 difference.

A detached thermometer in the tent stood at 72°.

Comparison of the second Series.

Observation at the top of the mount at c.

	Barom. N° 2. above at c. In. Pts.	Therm. attached.	Therm. detached.
	27.7025	73.4	64.0
Correct for the Diff. of the two attached therm.	— 50		
Barometer at the top,	25.6975	Log. 4098908	
———— below, —	28.3901	Log. 4531669	

Difference, or fall of the quickfilver, — } 2.6926 Diff. of Log. 432.751 { approx. height in English fathoms,

Correct for 28°.8 of heat, — — + 27.787

Corrected height in fathoms, — — 460.538

× 6

Height in feet by the barometer — 2763.228
——————— by the trig. method, — 2831.3

Difference, or error of the barometer $\frac{248}{10000}$, — 68.1

Observation below at A.

	Barom. N° 1. below at A. In. Pts.	Therm. attached.	Therm. detached.
	28.3940	71.6	73.0
Correct for the diff. of barometer,	— 39		64.0 heat at c.
	28.3901		68.5 mean heat. 39.7 standard temperature.

+ 28.8 difference.

A detached thermometer in the tent stood at 69°.

During these observations the wind was S.W.; the weather hazy, accompanied with a little thunder.

Comparison of the third series.

Observations at the top near c.

	Barom. N° 2. above at c. In. Pts. 25.6900	Therm. attached. $\overset{\circ}{6}9.7$	Therm. detached. $\overset{\circ}{6}2.0$
Correct for the diff. of the 2 attached therm. 1°.4, }	+ 38		

Barometer at the top,	25.6938	Log. 4098283
————— below, —	28.3896	Log. 4531593

Difference, or fall of the } 2.6958 Diff. of Log. 433.310 { Approx. height in
quickfilver, — } fathoms.

Correct for 27°.5 of heat, — — + 26.582

Correct height in fathoms, — — 459.892
× 6

Height in feet by the barometer, — 2759.352
————— by the trig. method, — — 2831.3

Difference, or error of the barometer $\frac{254}{10000}$, — 71.9

Observations below near to A.

	Barom. N° 1. below at A. In. Pts. 28.3935	Therm. attached. $\overset{\circ}{7}1.1.$	Therm. detached. $\overset{\circ}{7}2.5$ 62.0 heat at c.
Correct for the diff. of barometer, }	— 39		
	28.3896		67.2 mean heat. 39.7 standard temperature.
			+ 27.5 difference.

A detached thermometer in the tent stood at 65°.

Thefe

Thefe obfervations then feem to prove that the baro-metrical rules were a little defective as to the true ratio be-tween the gravities of air and quickfilver, *viz.* in the value of an inch of quickfilver in the torricellian tube, expreffed in inches of the atmofphere with a given tem-perature. The firft comparifon gives for this error in defect − 19.8 feet in every 1000 feet; the fecond, 24.0 feet; and the laft, 25.4 feet: the mean of the three is 23.1 feet; and by fo much we may conclude that thefe rules, in greater heights alfo, will give the difference of elevation too little, *viz.* by $\frac{1}{43}$ nearly [(m)]. But it will be fair to make the experiment.

(m) Left any fufpicion fhould arife of a difagreement between the actual meafures taken by Mr. DE LUC and myfelf, I may obferve, that the mean refult of three obfervations, which I made independently of each other on the height of the Pitton or point c above the lake of Geneva, agree with the mean refult of Mr. DE LUC's operation from the levelling and the quadrant, to lefs than twelve inches; a greater correfpondency than which cannot be expected: and this was the true reafon why I chofe the fame fpot he had pitched upon. " *Le rocher* " *ifolé, qui domine toute la montagne.*" As a further confirmation, I compared his ftandard fteel rod of twelve Paris inches, which his brother obligingly furnifhed me with, with my brafs one, and found twelve inches on Mr. DE LUC's rule was on my rule, with 71° of heat,   —   —   12.784 Eng. inches.

Correction for the difference of expanfion between } brafs and fteel with 16° of heat,   —   —   } + 07

Length of Mr. DE LUC's French foot with 55°, —   12.7847
True length of the French foot *(vide* Phil. Tranf.)   12.7890

Error or difference from the true Paris foot   —   —,0043 = $\frac{1}{3000}$ nearly.

The

The Mole is a convenient, infulated mountain, fituated about eighteen miles eaft of Geneva, and rifing near five thoufand feet above the lake, much higher than any body, that I know of, has ever made thefe experiments at, with the required precifion. On this fummit I determined to confirm or correct my difcovery, and communicated my intentions to Mr. DE SAUSSURE, a very ingenious gentleman of this place, and well fkilled in various parts of natural and experimental philofophy, who gave me all the information neceffary, and obligingly promifed to accompany me, as did alfo Mr. TREMBLEY, affiftant to Mr. MALLET, well known in the aftronomical world. This expedition was undertaken in the latter end of Auguft and beginning of September. I fhall here beg leave to fet the reader down at the bottom of the mountain, and flatter myfelf he will accompany me to the top. It was about five in the afternoon when we left St. Joire, a wretched little village at the foot of the mountain to the eaft, and where we had dined in a moft miferable *auberge*, preparing to afcend the fummit on foot, being feven or eight in company, including guides and fervants, who carried my inftruments, provifions, &c.; the former confifting of the equatorial, the barometer, different thermometers, electrical balls, an hygrometer, and a dipping-needle; together with another barometer

of Mr. DE LUC's conftruction, a variation-needle, a level belonging to Mr. DE SAUSSURE, and a tent. Thus accoutered we proceeded up an afcent, not however very fteep, for three hours and a half without intermiffion, the path leading in a fpiral kind of direction, very rugged and full of loofe pieces of rock that are brought down with the melting fnows, paffing through romantic woods of fine firs and other trees, interfperfed here and there with a thin foil of excellent pafture. Before we arrived at the hut, where we were to fleep (for our intention was to lay upon the mountain that night, in order to have the more time the next morning for our operations) having walked on a little too far before, we loft fight of our guides. We called feveral times, but were never anfwered:——the night was now coming on; a kind of fog appeared, with fmall rain; our fituation became fomewhat embarraffing. We called again, but were anfwered by nothing but an echo, the place being a moft profound folitude. We began now to confider ourfelves as loft. Mr. DE SAUSSURE, though he had been feven or eight times before upon the mountain, found himfelf in doubt concerning the way; but after a fhort dilemma thought it beft to proceed. We did; and now began to perceive at a diftance fome little huts or hovels indiftinctly: a few more fteps affured us we were right, and about nine

o'clock

o'clock we had the good luck to find ourselves at the very hovel, where we were to rest that night. I own I now found myself quite contented, though I did not at all know what kind of place I was going to enter. It proved to be a little hut made of boards, consisting of one apartment only, eighteen or twenty feet square, and about twelve high in the center, without any windows or chimney for the smoke, except what was made by the holes in the roof, and the interstices between the boards at the sides, which were rudely put together, scarce closer than park-palings, affording an equal entrance to the wind, rain, and snow; for as these hovels are inhabited only for about four months in the summer, they are constructed without the least mortar or cement in the world; an humiliating witness this, how simple the architecture which nature and necessity suggest. On entering we found a comfortable fire, and the little *cabane* inhabited by a couple of Alpine shepherdesses and their two cows, on whose whey and some very coarse bread they wholly subsisted, not discontented but even proud of their lot; and who, out of a singular species of contempt, call the inhabitants of the plain *mange-rotis*, that is, eaters of roast-meat. Their language too was different; not French nor Italian, but partaking something of both; or, as I have been since informed, a corruption of the ancient Celtic.

A few

A few minutes after our arrival our guides rejoined us: it was now night, and in this rather too artlefs habitation we were obliged to lay in a little loft over the cows, our beds fome leaves and clean hay, and my bolfter my portmanteau[n]. I had had the caution to bring fome fheets with me, and, being a little tired with my walking, flept five hours pretty foundly, though much ftarved, having no other curtains than what this wooden canopy afforded, through which the ftars fhone moft brilliantly. Between four and five we arofe; found the heavens beautifully ferene, and, having eaten fome of our provifions, left this habitation, which might be fituated about two-thirds of the way up the mountain; and beginning our march about half after five reached the fummit a quarter before feven; but not without a good deal of climbing, and fometimes up an afcent of near 40° for feveral hundred feet. One of my fervants, before he got half way, found his head turn round, and himfelf fo giddy, at the height and precipices (a frequent effect in thefe fort of places) that he was obliged to return to the hut. In the afcent I faw the Sun rifing behind one of the neigh-

[n] ———————————— Frigida parvas
   Præberet fpelunca domos, ignemque, laremque,
   Et pecus, et dominos communi clauderet umbrâ;
   Sylveftrem montana torym cùm fterneret uxor
   Frondibus et culmo.   JUV. Sat. vi.

bouring

bouring alps with a moft beautiful effect, and the fhadow of the mountain we were then upon extended fifteen or twenty miles weft. We had now reached the fummit; and there my curiofity finifhed in aftonifhment. I perceived myfelf elevated 6000 feet in the atmofphere, and ftanding as it were on a knife-edge, for fuch is the figure of the ridge or top of this mountain; length without breadth, or the leaft appearance of a plain, as I had expected to find. Before me an immediate precipice, *à pic*, of above 1000 feet, and behind me the very fteep afcent I had juft now mounted. I was imprudently the firft of the company: the furprize was perfect horror, and two fteps further had fent me headlong from the rock.

On this fpot, with fome difficulty, we fixed the inftruments, and commenced our operations, after fome time fpent in admiration at the profpect, and familiarizing myfelf to the fcene. Before me, at fome diftance, was fpread the plain in which lay Geneva and the lake; behind it rofe the Dole, and the long chain of Mont Jura as far as the fort La Clufe, which we entirely commanded, as well as fome of the country beyond it. A little to the left, and much nearer, lay Mont Saleve, which from this height appeared an inconfiderable hill: to the right and left nothing but immenfe mountains, and pointed rocks of every poffible fhape, and forming tremendous precipices. In the

vale beneath, ſeveral little hamlets, and the moſt beautiful paſturages, with the river Arve winding and ſoftening the ſcene; from whence aroſe a thick evaporation, collecting itſelf into clouds, which on the lake, that was quite covered with them, had the appearance of a ſea of cotton, the Sun-beams playing in the upper ſurface of them with thoſe tints that are ſeen in a fine evening. To the ſouth-weſt appeared the lake of Annecy; behind us, taking up one-fifth of our horizon, lay the Glacieres, and amongſt them, towering above all the reſt, ſtood Mont Blanc. The circumference of the horizon might be about 200 Engliſh miles; and, though not one of the moſt extenſive, yet certainly one of the moſt varied in the world. From this ſpot the clouds had a ſtriking appearance to an inhabitant of the plain; very few of them at above one-fifth of the height that we were now at; not governed by the wind, but moving in every poſſible direction; ſome of them ſeemed creeping along the ground, whilſt others were riſing perpendicularly between the hills. And I may here remark, that from Geneva I have obſerved the clouds were generally three days in the week below the ſummit of Mont Saleve; ſo that the ordinary region of theſe vapours ſeems to be at that height in the atmoſphere, where the barometer would ſtand at about 26 inches in this climate.

While

While at the top of the Mole, I was very fenfible of the cold, there being a brifk wind, which, though fouth, came over the mountains of ice, and was very keen; infomuch that, about two hours after I had been there, I nearly loft the ufe of my fingers, and found my lips much affected and parched from the tranfition, having been a good deal heated in afcending with two waiftcoats and a great coat on. The thermometer, however, when I firft mounted, ftood no lower than 48°. I muft here afk pardon for this long digreffion, which I have ventured to tranfcribe from my journal written upon the fpot.

To return then to the obfervations. After what has been faid refpecting thofe on Mont Saleve, it will fuffice here to mention, that by repeated meafurements I determined the horizontal length of the bafe 1, 2 (fee the chart) to be = 1250 ft. 3.9 inch; the ∠ at 1 = 95° 37′ 28″; ∠ at 2 = 77° 48′ 53″; and the ∠ at 3 = 6° 33′ 49″. The mean corrected angle of elevation of 3 from 1 = 21° 29′ 34″; ditto of 3 from 2 = 21° 3′ 41″; and laftly, the elevation of 2 from 1 = 0° 47′ 24″.

Thefe

	Feet.
Thefe obfervations give for the length of the fide 1, 3, —	10691.9
— — — — 2, 3, —	10886.7
Height of 3 above 1, — — —	4212.8
———— 3 above 2, — — — —	4194.8
———— 2 above 1, — — — —	17.2
And confequently, 3 above 1 deduced from the obfervation at 2, —	4212.0
And laftly, the mean height of 3 above 1 from the determination at each end of the bafe, — — —	4212.4

The difference in height, however, between the two barometers was only 4211.3 feet.

Here follow the barometrical obfervations [o], and their reduction.

[o] Made between the hours of eight and twelve, in the open air and not in the tent, which could not be pitched on accout of the fmallnefs of the plain at the fummit; a brifk fouth wind, but fair. The barometer was fcreened by an umbrella.

Comparifon of the firft feries on the Mole.

Obfervation at the top at 3.

	Barom. N° 2. above at c. In. Pts.	Therm. attached.	Therm. detached.
	24.1437	57.0	54.8
Correct for the Diff. of the two attached therm. 3°.4,	+ 88		
Barometer at the top,	24.1525	Log. 3829621	
——— below, —	28.1253	Log. 4490971	

Difference, or fall of the quickfilver, — } 3.9728 Diff. of Log. 661.350 { approx. height in fathoms.

Correct for 18°.6 of heat, — — + 27.431

Corrected height in fathoms, — — 688.781
× 6

Height in feet by the barometer — 4132.686
——————— by the geometrical meafurement, 4211.3

Difference, or error of the barometer, — 78.6 = $\frac{187}{10000}$.

Obfervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1295	60.4	61.9
Correct for the diff. of barometer,	— 42		54.8 heat at 3.
	28.1253		58.3 mean heat. 39.7 ftandard temperature.
			+ 18.6 difference.

Comparifon of the fecond Series.

Obfervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1420	56.9°	56.0°
Correct for the diff. of the two attached therm. 3°.5, }	+ 91		
	24.1511	Log. 3829369	
	28.1258	Log. 4491049	

Difference, or fall of the quickfilver, — } 3.9747   Diff. of Log. 661.680   { approx. height in fathoms.

Correct for 19°.2 of heat, — —   + 28.330

Correct height in fathoms, — —   690.010

           × 6

Height in feet by the barometer, —   4140.06

——— by the geom. method, —   4211.3

Difference, or error of the barometer,   — 71.2 = $\frac{169}{10000}$.

Obfervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1300	60.4°	61.8°
Correct for the diff. of barometer, }	— 42		56.0 heat at 3.
	28.1258		58.9 mean heat.
			39.7 ftandard temperature.
			— 19.2 difference.

Com-

Comparifon of the third Series.

Obfervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1670	56.0	56.0
Correct for the diff. of the two attached therm. 4°9. }	+ 127		☾ 57.5 (p)
	24.1797	Log. 3834509	
	28.1278	Log. 4491358	

Difference, or fall of the quickfilver, }	3.9481 Diff. of Log. 656.849	{ Approx. height in fathoms.
Correct for 19°8 of heat,	— —	+ 29.0
Correct height in fathoms,	— —	685.849
		× 6
Height in feet by the barometer,	—	4115.094
—————— by the geom. method,	—	4211.3
Difference, or error of the barometer,		— 90.2 = $\frac{-332}{10000}$.

Obfervation below at 1.

	Barom. N° 2. below at 2. In. Pts.	Therm. attached.	Therm. detached.
	28.1320	60.9	63.0
Correct for the diff. of the barometer, }	— 42		56.0 heat at 3.
	28.1278		59.5 mean heat. 39.7 ftandard temperature.
			19.8 difference.

(p) In this column for the detached thermometer at the top of the mountain, in this and the following obfervations, are inferted two numbers, the upper one expreffing the heat in the fhade; and the lower one, with this mark ☾ prefixed, the heat in the Sun. The computation, however, is made from the former; this may ferve to fhew the difference.

Compariſon of the fourth ſeries.

Obſervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1780	57.2	56.0
Correct for the diff. of the two attached therm. 4°6,	+ 119		☉ 57.5
	24.1899	Log.	3836341
	28.1318	Log.	4491976

Difference, or fall of the quickſilver,	3.9419	Diff. of Log.	655.635	{ approx. height in fathoms.
Correct for 20°.3 of heat,	—	—	+ 29 678	
Correct height in fathoms,	—	—	685.313	
			× 6	

Height in feet by the barometer,	—	4111.878
——————— by the geom. method,	—	4211.3

Difference, or error of the barometer,   — 99.4 = $\frac{235}{10000}$.

Obſervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1360	61.8	63.9
Correct for the diff. of the barometer,	42		56.0 heat at 3.
	28.1318		60.0 mean heat.
			39.7 ſtandard temperature.
			+ 20.3 difference.

Com-

Comparifon of the fifth feries.

Obfervations at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1840	59.6	57.0
Correct for the diff. of the 2 attached therm. 2°.8,	+ 73		⊙ 59.3

	24.1913	Log.	3836592
	28.1308	Log.	4491820

Difference, or fall of the quickfilver,	3.9395	Diff. of Log. 655.228	approx. height in fathoms,
Correct for 20°.8 of heat,	—	—	+ 30.391

Correct height in fathom,	—	—	686.619
			× 6

Height in feet by the barometer,	—	4113.714
——————— by the geom. method,	—	4211.3

Difference, or error of the barometer,	— 97.6 = $\frac{231}{10000}$.

Obfervations below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1350	62.4	64.0
Correct for the diff. of the barometer,	— 42		57.0 heat at 3.
	28.1308		60.3 mean heat.
			39.7 ftandard temperature.
			+ 20.8 difference.

Compariſon of the ſixth ſeries.

Obſervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1900	$\overset{\circ}{61}.0$	$\overset{\circ}{57}.0$
Correct for the diff. of the two attached therm. 1°6, }	+ 41		⊙ 60.0
	24.1941	Log. 3837095	
	28.1268	Log. 4491204	

Difference, or fall of the quickſilver, } 3.9327 Diff. of Log. 654.157 { approx. height in fathoms.

Correction for 20°6 of heat,   —   —   + 30.048

Correct height in fathoms,   —   —   684.157

$\times$ 6

Height in feet by the barometer,   —   4104.942
————————— by the geom. method   —   4211.3

Difference, or error of the barometer,   —106.4 $= \frac{252}{10000}$.

Obſervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1310	$\overset{\circ}{62}.6$	$\overset{\circ}{63}.6$
Correct for the diff. of the barometer, }	— 42		57.0 heat at 3.
	28.1268		60 3 mean heat.
			39 7 ſtandard temperature.
			20.6 difference.

To

To collect these last experiments in one point of view.

Feet.

				Feet.
The 1st series gives for the error on every 1000 ft.				18.7
2d,	—	—	—	16.9
3d,	—	—	—	22.8
4th,	—	—	—	23.5
5th,	—	—	—	23.1
6th,	—	—	—	25.2
The mean error,		—		21.7

which agrees within two feet in a thousand with the determination on Mont Saleve. This result then justifies my conclusion (in p. 556.) and proves that either the proportional gravity of air and quickfilver is now different from what it was, when M. DE LUC made his experiments, *viz.* from 1756 to 1760; or that his or my observations are defective. That my trigonometrical measurements were sufficiently exact, *viz.* to within two or three feet, I think I have already shewn; and even that his were also. Within what limits my barometrical errors are to be found is not difficult to determine from what has been before premised. That the scale of Mr. DE LUC's barometer was less accurate than mine, is, I think, without a doubt; and indeed he never attempted a division less than $\frac{1}{16}$th of a French line, or about $\frac{5}{1000}$

of

of an inch Englifh: and yet when I confider the number of his obfervations, and the unexampled diligence and care with which he made them, I am obliged to attribute the difference of our refults to fome other caufe than that of inaccuracy. If then future experience fhould demonftrate, that the denfity of the atmofphere with a given heat is invariable, or nearly fo; while the preffure of a whole column of it continues the fame, we may perhaps fearch for the caufe of our difagreement from hence, *viz.* the barometers of Mr. DE LUC were not fufficiently near each other in an horizontal direction: mine were feparated from two to three miles; and his, I believe, at double or triple that diftance. It may be fufpected, I am well aware, that the fyphon conftruction of Mr. DE LUC's barometer might occafion this difference: let us fee whether this be the cafe. Mr. DE SAUSSURE (whofe inftrument was of Mr. DE LUC's conftruction, and made, as I underftood, under his infpection) obferved at the top of the Mole, or at leaft nearly on the fame level with my barometer, as follows:

And

	Barometer		Therm. attached.	Therm. det.
	In.	L. 16ths.	DE LUC's fcale.	REAU's fcale.
	22	8 0	$+1°+$	$+10°-$

And in Englifh meafure and FAHREN-
HEIT's fcale, — — } 24.1570   56   54.2

Mr. DE SAUSSURE's barometer ordinarily }
ftands higher than mine N° 2. by (q), } —.0117

Correct for the diff. of our attached therm. 1°,   + 26

Mr. DE SAUSSURE's barometer corrected,   24.1479
My barometer N° 2. fee the firft feries,   24 1437 —   57   54.8:

Difference,   —   —   +.0042 wholly inconfiderable.

Our barometers may therefore be faid to have agreed exactly.

Mr. DE SAUSSURE made a fecond comparifon juft before we left the top of the mountain, which proved as follows.

	Barometer		Therm. attached.	Therm.
	In.	L. 16ths.	DE LUC's fcale.	detached.
	22	8 8	$+4°$	$+11\frac{2°}{3}$

Or reduced to Englifh meafure and fcale,   24.2014   61.7   57.9

Mr. DE SAUSSURE's barometer ftands }
higher than mine N° 2. — } —.0117

Corr. for the diff. of our attached therm. 0°.7, —.0018.

Mr. DE SAUSSURE's barometer corrected,   24.1879
My barometer N° 2. fee the fixth feries,   24.1900   61.0   57

Difference,   —   —   —0.0021

So that, in the firft comparifon, his barometer at the top of the Mole ftood higher than mine by +,004 inch; and in the laft, lower by —,002; the mean is higher by

(q) This we found by comparifons at the bottom of the mountain.

+,001

+,001, equal to about 10 inches in deducing the height of the mountain, a quantity wholly to be neglected. Finally, the mean of Mr. DE SAUSSURE's obſervations gives the defect of Mr. DE LUC's rules 21.9 in a thouſand. The conſtruction of the barometer had therefore no influence on this difference. But further, while Mr. DE SAUSSURE obſerved the height of the barometer on the Mole, Mr. DE LUC, the brother made a correſponding obſervation with a ſimilar inſtrument at Geneva. I ſhall relate this obſervation, computed after Mr. DE LUC's manner.

Mr.

In. L. 16ths.

Mr. DE SAUSSURE, at 4 feet below the summit of the Mole,	22	8	o	
Mr. DE SAUSSURE's barom. stands higher than Mr. DE LUC's ordinarily by,		+	1½	
Thermometer attached + 1°,		—	o¾	

Heat of the air.

						REAUM. Therm.	DE LUC's Therm.
			16ths of a line.	Log.			
Correct height on the Mole,	22	8	o¾ = 4352.¾	6387587		+ 10	— 15⅔

Mr. DE LUC, 78 feet above the lake,	27	0	0		
Therm. attached + 6°,		—	6		

	26	11	10 = 5178	7141620	+ 15	— 4

Difference of the Log.	—	—	754.033	Sum — 19⅔
19⅔ × $\frac{750433}{1000}$ = the correction for the temperature,			—14.854	

Correct height in French toises,	—		739.179	
			× 6	

Height in French feet,	—	—	4435.074	
Mr. DE LUC's barometer above the lake of Geneva,			+ 78.	
Mr. DE SAUSSURE's barometer below the summit of the Mole,	—	—	+ 4.	

And consequently, the summit of the Mole above the lake, in French feet,	—	—	4517.	
Which reduced to English feet is,	—	—	4814.	
But, by a mean of my trigonometrical operations, this height is (*vide* chart)	—	—	4883.	

Difference, or error of the barometrical rules,	—69. = $\frac{14}{1000}$.	

This last observation serves at least to shew, that the
error I am contending for is on the defective side, though
it gives the quantity of it somewhat less, but by no means
deserves that confidence which the other comparisons do;
for, besides that this single observation may be concluded
: less

leſs decifive, the trigonometrical meaſurement is alſo leſs accurate from the diſtance; and, laſtly, to ſuppoſe the ſtate of the atmoſphere preciſely the ſame with reſpect to weight in two places twenty miles aſunder, is, I am afraid, a *poſtulatum* too hazardous to grant. I therefore ſay, that all theſe obſervations confirm the ſame truth, that the atmoſphere is lighter than Mr. DE LUC preſumed it. What had already been done may ſeem ſufficient for the eſtabliſhment of this fact; for I have always held, that a few obſervations, well made and faithfully related, do more in the interpretation of nature, than a multitude of crude, careleſs, and immethodical experiments. But I have not done: I wiſhed to put this matter out of all doubt, and accordingly undertook another expedition to the ſummit of Mont Saleve, on the 18th of September, and in a colder temperature: the experiments then made, with their reſults, were as follows:

The difference of actual height by the two barometers was 2828.9 feet, the barometer N° 1. ſtanding higher than N° 2. by +,0038 inch, when compared at the bottom of the mountain.

Comparifon of the firft feries.

Obfervation at the top of the mountain.			Obfervation at the bottom.		
Barom. N° 2. at the top. In.	Therm. attached.	Therm. detached.	Barom. N° 1. below. In.	Therm. attached.	Therm. detached.
25.6533	58.0	56.2	28.4040	58.1	58.8

Feet.

This gives for the height barometrically, 2755.6
But the true height was, —— 2828.9

Difference, or error of the barometers, $-73.3 = \frac{259}{10000}$.

Comparifon of the fecond feries.

Obfervation at the top of the mountain.			Obfervation at the bottom.		
Barom. N° 2. at the top. In.	Therm. attached.	Therm. detached.	Barom. N° 1. below. In.	Therm. attached.	Therm. detached.
25.6550	56.2	57.0	28.4040	58.5	60.8

Feet.

This gives for the height barometrically, 2754.9
But the true height was, —— 2828.9

Difference, or error of the barometers, $-74.0 = \frac{262}{10000}$.

Comparifon of the third feries.

Obfervation at the top of the mountain.			Obfervation at the bottom.		
Barom. N° 2. at the top. In,	Therm. attached.	Therm. detached.	Barom. N° 1. below. In.	Therm. attached.	Therm. detached.
25.6620	56.2	57.2	28.4040	59.3	62.0

Feet.

This gives for the height barometrically, 2748.9

The height by the trigon. method was, 2828.9

Difference, or error of the barometers, $-80.0 = -\frac{282}{10000}$

Comparifon of the fourth feries.

Obfervation at the top of the mountain.			Obfervation below.		
Barom. N° 2. at the top. In.	Therm. attached.	Therm. detached.	Barom. N° 1. below. In.	Therm. attached.	Therm. detached.
25.6600	56.4	57.4	28.4040	59.3	62.2

Feet.

This gives for the height barometrically, 2752.8

But the true height was,   —   2828.9

Difference, or error of the barometers, $-76.1 = \frac{269}{10000}$

In thefe comparifons I have not inferted the whole of the computation, as that may eafily be made by any perfon at leifure. Finally, the mean of thefe four laft

feries

feries gives for the error on 1.000 feet, 26.8. I think I have now shewn, that the error actually exists; it remains that we determine precisely the quantity of it. For this purpose it will be proper to collect all the preceding observations in one point of view.

Table of the result of all the barometrical experiments.

Place of observation		True height trigonometrically.	Height by the barometers.	Mean heat.	Error in feet.	Error in 1000 feet.	Mean error in 1000 feet.
Mont Saleve,	1	2831.3	2775.2	69.4	— 56.1	—19.8	} —23.1
	2	——	2763.2	68.5	— 68.1	—24.0	
	3	——	2759 4	67.2	— 71.9	—25.4	
At the Mole,	1	4211.3	4132.7	58.3	— 78.6	—18.6	} —21.7
	2	——	4140.1	58.9	— 71.2	—16.9	
	3	——	4115.1	59.5	— 96.2	—22.8	
	4	——	4111.9	60.0	— 99.4	—23.5	
	5	——	4113.7	60.5	— 97.6	—23.1	
	6	——	4104.9	60.3	106.1	—25.2	
Mont Saleve,	1	2828.9	2755.6	57.5	— 73.3	—25.9	} —26.8
	2	——	2754.9	58.9	— 74.0	—26 2	
	3	——	2748.9	59.6	— 80.0	—28.2	
	4	——	2752.8	59.8	— 76.1	—26.9	

Mean of all, 23.6, and the temperature 61°.4.

The Mole, from two observations of Mr. DE SAUSSURE, —	4211.3	——	—	— 92.	—21.8	} —16.2
The same by Mr. DE SAUSSURE, and Mr. DE LUC, at Geneva,	4883.	4814.	—	— 69.	—14.	
According to Mr. DE LUC's own observations, see *Recherches sur l'Atmosphere,* the Mole,	4882.8	4860.	—	— 22.8	— 4.7	
the Dole,	4292.7	4210.	—	— 82.7	—19.5	
the Buet,	8893.6	8770.	—	—123.7	—13.9	
Mt Blanc,	14432.5	14093.		—339.5	—23.5	

The

The titles of the columns are ſufficiently clear to make a farther explanation of this table unneceſſary; and it appears, I think inconteſtably, upon taking a mean of my thirteen obſervations (and I ſhall here conſider only my own) on Mont Saleve and the Mole, that this error is about $23\frac{1}{2}$ feet on every thouſand; that is, the rules of Mr. DE LUC give the height by ſo much too little. At the bottom of the foregoing table I have ſubjoined ſix other compariſons, ſome of them from Mr. DE LUC's own obſervations, as recorded in his valuable work; which however I muſt add, are certainly of leſs authority in this inquiry, as they were made with barometers a great way diſtant from each other, *viz.* near thirty miles: beſides which, the geometrical heights are, for the ſame reaſon, not ſo accurately aſcertained. I have, however, ventured to make what uſe I could of them, *viz.* to ſhew that theſe two give a reſult on the ſame ſide, though not exactly the ſame; and to urge the neceſſity of a certain vicinity in thoſe obſervations from whence a theory is to be deduced.

Shall I be permitted to adduce another proof, in confirmation of what has been advanced? When I firſt took up the conſideration of meaſuring altitudes in the atmoſphere with the barometer, and had heard only of Mr. DE LUC's labours, it occurred to me, that there was a

much

much more fimple method of arriving at this theory, than either he or I have fince purfued. It was this; to determine hydroftatically the fpecific gravities of air[r] and quickfilver, with a given temperature and preffure; the increafe of volume, or change of gravity, with a given increafe of heat being fuppofed to be known by the experiments of BOERHAAVE [s] and HAWKESBEE [t], which might be farther examined by fimilar ones; and prefuming that the geometrical ratio in the air's denfity, as you advance upwards from the earth's furface, had been fufficiently demonftrated[u]. For the proportional gravity of quickfilver to air will exprefs inverfely the length of two equiponderant columns of thefe fluids, that is, when the columns are taken infinitely fmall[x]. With thefe

(r) It may feem particular that I fhould propofe an experiment fuppofed to be very well known, and which hardly any elementary treatife on chemiftry or experimental philofophy will not furnifh us with an example of; the weight of a given quantity of air. BOYLE, HALLEY, HAWKESBEE, HALES, each of them have tried it, and many others fince their time: but the misfortune is, all thefe experiments have been but grofs approximations, without due attention to the heat; and yet the determination of HAWKESBEE feems to have been followed by one-half of Europe in Pneumatical refearches. Indeed I only know of one experiment that has the leaft title to precifion, and that is Mr. CAVENDISH's, briefly related in the LVIth volume of the Philofophical Tranfactions.

(s) Elementa Chemiæ.

(t) Phyfico-mechanical Experiments.

(u) COTES's Hydroftat. Lectures, *et alibi.*

(x) I am not forry to anticipate the reader's remark here, that this obfervation is not new; fince I find that I have been treading the fame fteps with

Mr.

thefe ideas I made the following experiment. I caufed a glafs veffel to be blown fomething like a Florence flafk, or rather larger; to the neck of this was adapted a brafs cap with a valve opening outwards, and made to fcrew on or off, together with a male fcrew, by which it was fixed to an excellent pump of Mr. NAIRNE's conftruction, and exhaufted of its air, or at leaft rarified to a known degree: the veffel was then carefully weighed with a fenfible balance, and again after the air was re-admitted; the difference gave the weight of the air that had been exhaufted. After having repeated this two or three times, the veffel was exactly filled with water as far as the valve, which had been the term of capacity for the air; this was done by fcrewing on the cap till the fuperfluous water oozed all out, and upon inverting the veffel there appeared not the leaft fign or bubble of air; I therefore concluded, that the volume of water was precifely the fame as had been the volume of air, a circumftance that fhould be accurately attended to. It was then carefully weighed, and compared with its weight when full and deprived of its air. It will readily be feen, that I had then the fpecific gravity of the two fluids, upon fuppofition that the figure of the glafs had not altered

Mr. BOYLE and Dr. HALLEY, who both made ufe of this method; the one with a view to determine the limits of the atmofphere; and the other the height of Snowden.

by

by preſſure during the experiment; and this effect may be preſumed to have been the moſt ſenſible, when the veſſel was filled with water, the preſſure at that time being from within. To aſſure myſelf of this, I let in a ſmall quantity of air, which formed a bubble of about one-third of an inch in diameter, and upon immerging the glaſs in another veſſel of water, whereby the preſſure within was counterpoiſed by a preſſure without, the bubble ſeemed to contract itſelf by a quantity, as I found afterwards, equal to about two grains in weight, or $\frac{1}{8000}$ of the whole contents. I therefore concluded, that this correction was hardly worth taking notice of, and ſtill leſs the effect from external preſſure when the glaſs was exhauſted. At every operation the height of the baro-meter and thermometer (placed cloſe to the veſſel when the air was weighed) was noted down, together with the height of the pump-gage, which, compared with the barometer in the room, ſhewed the quantity exhauſted. The reſult of the experiment was as follows, the baro-meter in the room ſtanding at 29.27 inches, and the heat of the room 53°.

	Feet.
The bottle empty or exhauſted till the gage ſtood at 29.15 inches weighed (determined from four different trials, and the balance turning with $\frac{1}{15}$ of a grain) — — —	2657.40
Increaſe of weight when filled with air, from four trials certain to $\frac{1}{10}$ of a grain — — — —	+16.13
Bottle filled with water, whoſe heat was 51°, — —	16220.00
Weight of the water, excluſive of the bottle, — —	13562.60

But the bottle was exhauſted only in the proportion of 29.15 inches to 29,27 inches; therefore if a perfect *vacuum* could have been made, the difference of weight would have been 16.22 grains inſtead of 16.13 grains. Again, the water was colder than the air by 2°; the one being 53°, and the other only 51°: now water, from former experiments, I find to expand about $\frac{3}{10000}$ with 2° of heat; therefore, if the water had been of the ſame temperature with the air that was examined, the weight of an equal volume would have been only 13558,5 grains; and laſtly, 13358.5 divided by 16.22 gives 836 [y], and by ſo much is water heavier than air in theſe circum-ſtances.

[y] HAWKESBEE's experiments made the air 850 lighter than water, the baro-meter being at 29 7; and Dr. HALLEY ſuppoſed it about 800. Mr. CAVEN-DISH, in weighing 50 grains of air, when the barometer was at 29$\frac{3}{4}$, and the thermometer at 50°, concluded the ſpecific gravity of air to be about 800 alſo. Now my experiment, reduced to the ſame circumſtances with his, would give 817 for this gravity, no great difference in an affair of ſuch delicacy.

By

By former experiments I find the fpecific gravity of the quick-
filver of my barometers, compared with rain-water in 68° } 13.606 to 1
of heat, as,   —   —   —   —
And 68°—53°=15°, correct therefore for 15° of expanfion of } +.018
quickfilver,   —   —   —   —
Correct for 15° of expanfion of air,   —   —   —.031

True fpecific gravity of quickfilver, with 53° of heat,   13.594
Which multiplied by the fpecific gravity of air,   —   × 836

Gives for the comparative gravity of quickfilver and air, when } 11364.6
the barometer is at 29.27, and the thermometer 53°,   —

Feet.

And laftly, $\frac{1}{10}$th of an inch of quickfilver, when the barometer ftands at }
29.27 inches (*viz.* from 29.22 inches to 29.32 inches) with the tem- } 94.7
perature 53°, is equal to a column of the atmofphere of,   —
This quantity, according to my barometrical obfervations, is,   —   93.83
———————————————— to Mr. DE LUC's rules,   —   —   91.66

We fee here then that the ftatical experiment agrees with the refult of my barometrical ones to within about 11 inches in 100 feet, and I am not fure that it is not ftill capable of much farther precifion; and though per-haps alone it might carry with it, to fome perfons, a lefs conclufive teftimony, who reluctantly reafon from the little to the great, yet, in conjunction with what has been before fhewn, I think it has confiderable weight; and I am the lefs inclined to reject fuch an indirect method of proof, as I have the great authorities of HALLEY and NEWTON on my fide [z].

I have

(z) " Ce qu'il y a d'effentiel à obferver ici," fays Mr. DE LUC, " et vrai-
" ment digne de remarque, c'eft que par la feule connoiffance des pefanteurs
" fpecifiques de l'air et du mercure, HALLEY eft parvenu à une regle très

I have thus endeavoured to ſhew then that the error
of the theory is $-\frac{236}{10000}$ when the temperature of the
air is 61°.4 (ſee the table of the reſult of the obſerva-
tions). It remains therefore, finally, that we deduce a
rule, the error of which ſhall be nothing, *viz.* to find
the temperature wherein the difference of the loga-
rithms of the heights of the barometer, taken to four
places of figures, will give the true difference of eleva-
tion in Engliſh fathoms. Previous to this inveſtigation,
with which I intend to conclude this paper, it will be ne-
ceſſary to remark, that by repeated experiments with the
barometer, I find a ſmall difference in the equation for
the expanſion of air by a change of temperature, and
even in that of quickſilver from the ſame cauſe, from
what Mr. DE LUC's obſervations have given it[a]. I ſhall

"approchante de celle, qu'un grand nombre d'obſervations du baromètre dans
"les Cordelières ont dicté depuis à M. BOUGUER: cependant malgré l'appui
"que ces experiences ſe prêtent reciproquement, on verra qu'elles etoient encore
"bien eloignées de fournir une regle generale." Rechenches ſur l'Atmoſphere,
ſect. 267.

(a) He indeed made his experiments on the atmoſphere itſelf with the
barometer, in order to dètermine the variations of its denſity; but ſince it
appears that the abſolute denſity of this fluid is different from what he ſup-
poſed it, it is no bold conjecture to preſume that the degree of its variation
ſhould be different alſo; and to aſcertain this point, I have preferred the
inſtrument above-mentioned to the method uſed by Mr. DE LUC, how direct
ſoever his may ſeem; for in determining minute quantities or equations, we
muſt not embarraſs ourſelves with the compound effect of too many cauſes at a
time.

not

not here trouble the reader with the experiments at large, too fimple in themfelves to deferve fuch a detail, unlefs a future occafion fhould render that neceffary, as the methods here ufed may be met with amongft HAWKESBEE's or Mr. BOYLE's experiments; and content myfelf with relating only the refult of the different trials.

1000 parts of air of the temperature of freezing and preffure of 30½ inches, increafed in volume by an addition of 1 degree of heat on FAHRENHEIT's thermometer as follows:

Obfervations.		Number of degrees the air was heated.	Expanfion for 1° in 1000ths of the whole.	
With the firft manometer,	1	14.6	2.30	Mean from the firft manometer 2.44.
	2	32.2	2.43	
	3	40.3	2 48	
	4	46.6	2.45	
	5	49.7	2.48	
	6	51.1	2.51	
	7	23.7	2.36	
	8	13.1	2.24	
With another manometer,	9	22.0	2.38	Mean from the fecond manometer 2.42
	10	28.0	2.50	
	11	21.5	2.34	
	12	30.1	2.44	
	13	22.6	2.44	

The

The mean of theſe two ſorts of obſervations, made with different inſtruments, is 2.43, *viz.* 1000 parts of the air at freezing become by expanſion from 1° of heat

Pts.   Pts.

equal 1002.43 or 1002.385 with the ſtandard temperature 39°.7. Mr. DE LUC's experiments reduced give

Pts.

this quantity equal 1002.23[b] (ſee Tranſ.). It may be imagined, that I ſhould have had a more accurate concluſion by making theſe obſervations in greater differences of temperature than what is ſhewn in the ſecond column of the above table; but it did not appear ſo to me. On the other hand, I found that it was abſolutely neceſſary that the ſame heat ſhould be kept up for ſome hours together, in order that I might be ſure that the air within the inſtrument, the glaſs tube that contained it, and the air without it, all had acquired the ſame

---

(b) It has generally been ſuppoſed, that air expands $\frac{1}{400}$ with each degree of the thermometer, commencing from the mean temperature 55°; and, in conſequence of this, aſtronomers have computed tables for correcting their mean refractions; but, upon reducing the reſult of my obſervations to the temperature 55°, we ſhall have the correction of the refraction for 1° $= \frac{243}{105539}$ or $\frac{1}{435}$. Now according to Mr. DE LUC this equation is $\frac{223}{103345} = \frac{1}{463}$, which would produce a difference of about 4″ in the corrected refraction, upon an altitude of 5°, with the temperature 35°. If my numbers may be ſuppoſed to deſerve equal confidence, the error of the tables in common uſe, in the above circumſtances, would amount to only half that quantity, and therefore probably will be thought ſcarce worth correcting. I have mentioned this in order to obviate the concluſions that have been drawn by ſome perſons from Mr. DE LUC's theory.

uniform

uniform temperature, which in my room I found not very easy to effect in heats greater than 70° or 80°. I have therefore preferred repeating the experiment with small differences of heat; but such, however, as will include almost all the temperatures in which barometrical observations are likely to be made, *viz.* from 32° to 83°.

It has been suspected, in consequence of some experiments made by a very ingenious member of this Society, that air does not expand uniformly with quicksilver; or that the degrees of heat shewn by a quicksilver-thermometer would be expressed on a manometer, or air-thermometer, by unequal spaces in a certain geometrical ratio. I do not deny this proposition; but I have also very little reason to assent to it, if I may trust my own experiments, which certainly evince that this ratio, if not truly arithmetical, is so nearly so as to occasion no sensible error in the measuring of heights with the barometer; and that is all I contend for. The small differences that are seen in the above table of this expansion, deduced from a mean of 14° or of 40°, I would attribute rather to the errors of observation than to any actual irregularity in nature. If, however, this progression be insisted upon, it should seem, that the degree of the air's expansion increases with an increase of heat; and that the difference of volume or density from 1° of

heat, any where within the limits above-mentioned, would be about one part in five thoufand from what I take it at a mean. I fhould not have infifted fo long on this circumftance, but in refpect to the known accuracy of the author of this hypothefis. Neither do I find any reafon to believe, that the expanfion of air varies with its denfity. I have tried air whofe denfity or preffure was equal to $23\frac{3}{4}$ inches, and alfo to forty inches; but the dilatation, with equal volumes and equal degrees of heat, was very nearly the fame in both cafes. I might add a great deal more on thefe manometrical experiments, but I am afraid it would be more tedious than ufeful. I proceed therefore to the expanfion of quickfilver.

This experiment was made with a tube, fomething like a thermometer, but confiderably larger than the ordinary fize, and open at one end; it was filled with quickfilver to a certain height, and then expofed to the temperatures of freezing and boiling repeatedly, the barometer being at 30 inches: the difference of the volume in each inftance was determined afterwards by accurately weighing the contents. I thus found, that if the quickfilver at freezing be fuppofed to be divided into 13119 parts, the increafe of volume by a heat of boiling water became equal to 208 of thefe parts $= \frac{10}{637}$, and $\frac{10}{637} \times \frac{1}{180} = \frac{1}{11466}$; and fuch would be the expanfion for

2   each

each degree of the thermometer, commencing from the freezing point, = 0,00262 inch on a column of 30 inches of the barometer, if the glafs had fuffered no expanfion during the experiment. This, however, has been found to be with 180° of heat = $\frac{1}{400}$ in folidity *(viz.* the cube of its longitudinal expanfion) and. $\frac{1}{400} \times \frac{1}{180} = \frac{1}{72000} = 0,00042$ inch, for the effect of the expanfion of the glafs for 1° upon a column of 30 inches; this added to the quantity before found, which was only the excefs of the greater expanfion above the lefs, gives for the true equation for each degree 0,00304 inch when the barometer ftands at 30 inches[c]. Mr. DE LUC's correction in this cafe was 0,00312; a difference fo fmall that I fhall take no notice of it as to its influence upon our obfervations. It may deferve a remark here, that this equation rigoroufly taken is variable according to the height of the thermometer; for 1°, which at

[c] It has been fufpected, and I believe will appear from very good obfervations, which however I never made myfelf, that the expanfion of quickfilver in the barometer is not directly as the heat fhewn by the thermometer, but in a ratio fomething different, owing to fome of the quickfilver being converted into an elaftic vapour in the *vacuum* that takes place at the top of the Torricellian tube, which preffes upon the column of quickfilver, and thus counteracts in a fmall degree the expanfion from heat. It does not, however, appear to be a confiderable quantity, not amounting to above one-fixteenth of the whole expanfion in a range of 40° of temperature; I fhall therefore venture to confider this equation as truly uniform, fince the error on ten thoufand feet would not amount to five.

freezing

freezing is $= \frac{1}{9891}$ of the whole volume, at the tempera-
ture 82° becomes $\frac{1}{9941}$, a difference indeed that may
fairly be neglected, and which I neglect myſelf; yet I
cannot help obſerving, in juſtice to Mr. DE LUC, that his
method of reducing his barometers always to the ſame
ſtandard temperature, was free from the error I am
ſpeaking of.

To conclude, the defect of Mr. DE LUC's rules being
ſuppoſed $\frac{236}{10000}$, or, which comes to the ſame thing, the
correction being $+ \frac{2417}{100000}$, when the temperature of the
air is 61°.4, and the true expanſion of the air for each
degree being $\frac{239}{100000}$ when the heat is 39°.7; required to
find the temperature wherein the difference of the loga-
rithms ſhall give the true height in Engliſh fathoms,
that temperature, according to Mr. DE LUC, being 39°.74,
and the expanſion $\frac{223}{100000}$.

Let T be the temperature 61°.4; s Mr. DE LUC's
ſtandard temperature; E the expanſion for 1°; e the ſame,
according to Mr. DE LUC; α the ſuppoſed correction of
the rules, and x the temperature ſought. We have then
the following formula, $\overline{T-s} \times \overline{E - e}^{(d)} - \alpha = s - x$, wherein
proceeding with the above numbers s−x comes out

(d) This ſign is negative, becauſe the aſſumed expanſion *e* is leſs than the
true one E, and conſequently tended to increaſe the apparent error of the rules;
had it been greater, *α* would have been +.

8°.50

8°.50, and consequently $x = 31°.24$ the temperature required; which, if it should be thought convenient, may be considered as the freezing point.

In the whole of the above inquiry I have taken no notice of the effect of gravity upon the particles of the air at different distances from the earth's center, which should doubtless enter into the account, and which would occasion the density of the atmosphere to decrease in a ratio something greater than the present theory admits of. In a height of four English miles Dr. HORSLEY finds (Phil. Transf. vol. LXIV.) that the diminution of density or volume from the accelerative force of gravity would be only $\frac{1}{500}$ part of the whole, or about 48 feet; and I may add to this, that this effect will be in the duplicate ratio of the heights, so that at one mile high it becomes only three feet. A like effect takes place also below the surface of the earth, as in measuring the depths of mines, &c. with this difference, that here it is but half the quantity; in the former instance gravity within the earth being simply as the distance from the center; they are both of them, however, circumstances that deserve no attention in practice.

This would be the place for me to enumerate the many desiderata, besides those already hinted at, that still remain for the perfection of this theory; such as the

laws of heat, that obtain in the different regions of the atmofphere; the effects of moifture, winds, the electric fluid, together with the weight and qualities of the air in different countries, &c.; that at the fame time that I am congratulating the prefent age on one of the moft brilliant difcoveries in natural philofophy, I may be underftood alfo to encourage every lover of fcience to ftill farther enquiries in a branch of knowledge no lefs ufeful than ingenious; particularly in a kingdom wherein, from its commercial interefts, and in confequence its many inland navigations, every improvement in hydroftatics, the art of levelling, or geometry, cannot but tend confiderably to the public benefit. The fources of fcience are not eafily exhaufted; multitudes of them remain wholly unexplored. If novelty can afford a charm, the path I am fpeaking of, till of late, has been the leaft frequented; witnefs the frefh, important truths in Pneumatical refearches that, from zeal and fafhion, every day's experience affords. I might here offer too a tribute of applaufe (and am fure in concert with this whole affembly) juftly due to the indefatigable labours of him whofe fteps I have purfued; but I am convinced he will rather hear me acknowledge our obligations to the ancients than any panegyric of himfelf. Be the benefit we receive from them our encouragement to proceed.

Multum

Multum egerunt, qui ante nos fuerunt, sed non peregerunt: multum adhuc restat operis, multumque restabit; nec ulli nato post mille sæcula præcludetur occasio aliquid adhuc adjiciendi." SEN. Epist. 64.

PART II.

IN the subsequent pages, which I have now the honour of laying before the Royal Society, I have drawn up, and I hope in a form the most commodious, the neceffary tables and precepts for calculating any acceffible heights or depths from barometrical obfervations, and without which I thought the preceding memoir would be incomplete; referring, however, to that for the proofs or elements from whence the tables have been computed. And herein I have endeavoured to adapt myfelf to the capacity of fuch perfons as are but little converfant with mathematical computations, but who may have frequent opportunities of contributing fomething to the advancement of fcience by experiments with this ufeful

inftru-

inſtrument, which is now become nearly in as common poſſeſſion as a pocket watch. I have induſtriouſly avoided the method of logarithms, propoſed by Dr. HAL-LEY, and adopted by Mr. DE LUC, both becauſe ſuch tables are not in the hands of every body, and becauſe I have perceived that many perſons of a philoſophical turn, though ſkilled only in common arithmetic, have been frightened by the very name: a method leſs popu-lar, however elegant, would have been leſs generally uſeful. To theſe tables is ſubjoined a liſt of ſeveral alti-tudes, as determined by the barometer: this will ſerve to ſhew the uſe I have made of the inſtrument, and will at the ſame time exhibit the level of a great number of places in France, Savoy, and Italy, and, as I think, be no improper ſupplement to exemplify the rules. It might have been expected that I ſhould have ſaid ſomething on the theory of barometrical obſervations, and have laid down the laws and principles on which it depends; but as that has been ſo amply done by other writers of in-conteſted authority, I ſhall content myſelf with inſerting only the following propoſitions.

1ſt, The difference of elevation of two places may be determined by the weight of the vertical column of the atmoſphere intercepted between them.

2. 2d, If

2d, If then the weight of the whole atmofphere at each place can be afcertained, the weight of this column, *viz*. their difference, will be known.

3d, But the height of the quickfilver in the barometer exprefles the total weight of the atmofphere in the place of obfervation; the difference, therefore, of the height of the barometer, obferved in two places at the fame time, will exprefs the difference of elevation of the two places.

4th, But further, the weight of this column of the atmofphere is liable to fome variations, being diminifhed by heat, and augmented by cold; and again, a fimilar alteration takes place in the column of quickfilver, which is the meafure of this weight.

5th, If then the degree of thefe variations can be determined, and the temperature of the air and quickfilver at the time of obfervation be known, the weight of this column of air, or the difference of elevation of the two places, will be concluded as certainly as if the gravity of thefe two fluids, with all heats, remained invariably the fame: this is the whole myftery of barometrical meafurement.

APPLICATION.

The height of the barometer in Engliſh inches at any two places at the ſame inſtant, and the heat (according to FAHRENHEIT's thermometer) to which it is expoſed, being known, together with the temperature of the air at each place, obſerved with a ſimilar inſtrument; required the difference of elevation of the two places in Engliſh feet.

R U L E.

Precept the 1ſt, With the difference of the two thermometers that give the heat of the barometer (and which, for diſtinction ſake, I ſhall call the attached thermometers[e]) enter table I. with the degrees of heat in the column on the left hand, and with the height of the barometer in inches, in the horizontal line at the top; in the common point of meeting of the two lines will be found the correction for the expanſion of the quickſilver

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(e) It is ſcarce neceſſary to remark, that, in order to make good concluſive obſervations, it is proper to be furniſhed with two barometers, and four thermometers; *viz.* one attached or inſerted in the frame of each barometer; and the other two detached from them, in order to take the heat of the open air; for it will ſeldom be found, that the thermometer in the frame of the barometer and that in the air will ſtand at the ſame point, and for a very evident reaſon.

by

by heat, expreffed in thoufandth parts of an Englifh inch; which added to the coldeft barometer, or fubtracted from the hotteft, will give the height of the two barometers, fuch as would have obtained had both inftruments been expofed to the fame temperature.

Precept the 2d, With thefe corrected heights of the barometers enter table II. and take out refpectively the numbers corresponding to the neareft tenth of an inch; and if the barometers, corrected as in the firft precept, are found to ftand at an even tenth, without any further fraction, the difference of thefe two tabular numbers (found by fubtracting the lefs from the greater) will give the approximate height in Englifh feet. But if, as will commonly happen, the correct height of the barometers fhould not be at an even tenth, write out the difference for one entire tenth, found in the column adjoining, intitled *Differences*; and with this number enter table III. of proportional parts in the firft vertical column to the left hand, or in the 11th column, and with the next decimal following the tenths of an inch in the height of the barometer *(viz.* the hundredths) enter the horizontal line at the top, the point of meeting will give a certain number of feet, which write down by itfelf; do the fame by the next decimal figure in the height of the barometer *(viz.* the thoufandths of an inch) with this difference,

ftriking

ſtriking off the laſt cypher to the right hand for a frac-
tion; add together the two numbers thus found in the
table of proportionable parts, and their ſum ſubduct
from the tabular numbers juſt found in table II.; the
differences of the tabular numbers, ſo diminiſhed, will
give the approximate height in Engliſh feet.

Precept the 3d, Add together the degrees of the two
detached or air-thermometers, and divide their ſum by
2, the quotient will be an intermediate heat, and muſt
be taken for the mean temperature of the vertical co-
lumn of air intercepted between the two places of ob-
ſervation: if this temperature ſhould be $31°\frac{1}{4}$ on the
thermometer, then will the approximate height, before
found, be the true height; but if not, take its difference
from $31°\frac{1}{4}$, and with this difference ſeek the correction
in table IV. for the expanſion of air, with the number of
degrees in the vertical column on the left hand, and the
approximate height to the neareſt thouſand feet in the
horizontal line at the top; for the hundred feet ſtrike off
one cypher to the right hand; for the tens ſtrike off two;
for the units three: the ſum of theſe ſeveral numbers
added to the approximate height, if the temperature be
greater than $31°\frac{1}{4}$, ſubtracted if leſs, will give the correct
height in Engliſh feet. An example or two will make
this quite plain.

EXAMPLE I.

Let the height of the barometer, obferved at the bottom of a mountain be 29.4 inches, the attached thermometer 50°, and the heat of the air 45°; at the fame time that at the top of the mountain the barometer is found to ftand at 25.190 inches, the attached thermometer at 46°, and the air-thermometer at $39°\frac{1}{2}$; required the height of the mountain in Englifh feet. Set the numbers down in the following order:

Obfer-

Obfervation at the bottom.

Barometer.	Therm. attached.	Therm. in the air.
In.		
29.400	50°	45°
	46.	

Diff. of the two attached thermometers,   4

Obfervation at the top.

	Barom.	Therm. attached.	Therm. in the air.
	In.		
	25.190	46°	39°½
Correct for the diff. of the two attached therm. *viz.* 4°,	+ 10.		.45
			2)84½(42¼ mean heat.
Height of the uppermoft barometer, reduced to the fame heat as the lowermoft, *viz.* 50°,	25.200		31¼ ftandard heat.
			11 difference.

			In.	Feet.
Correct for 11°, fee tab. IV.	Tabular number, fee tab. II. corresponding to,	25.200 =	6225.0	
on 4000 feet 106.9				
on 16 — + 5	The fame, corresponding to	29.400 =	2208.2	
or on 4016 +107.4	Approximate height in feet,		4016.8	
Correction for 11° of heat on 4016 feet, add, —		—	107.4	
Correct height of the mountain. —		—	—	4124.2

Now the difference of the attached thermometer 50°
and 46° is = 4°; and againft this number, in table I. in
the column for 25 inches (being the height of the baro-
meter in this cafe) I find 10, which added to 25.190, as
this barometer was the coldeft, gives 25.200 inches for
the

the height of the uppermost barometer reduced to the same heat as the lowermost: and in table II. opposite to 25.200 inches and 29.400 inches, I find respectively 6225.0 and 2208.2; their difference 4016.8 is the approximate height in feet. The degrees on the thermometer in the open air, 39°½ and 45° being then added together, and afterwards divided by 2, give for the mean temperature of these observations 42°¼, or 11° above the standard temperature, 31°¼: and lastly, the correction for 11°, in table IV. on 4000 feet I find = 106.9, and on 16 feet = 0.5; that is, 107.4 feet equal the whole correction, which added to 4016.8 gives 4124.2 feet for the correct height of the mountain.

EXAMPLE II.

Suppose the height of the barometer at the top of a rock had been observed at 24.178, the attached thermometer at 57°.2, the air-thermometer at 56°; the barometer below at 28.1318 inches, the attached thermometer 61°.8, the detached one 63°.9; what is the height of the rock?

Obser-

Obſervation at the bottom.

	Barometer.	Therm. attached.	Therm. detached.
	In.		
	28.1318	61°.8	63°.9
		57.2	
Difference of the two attached thermometers,		4.6	

Obſervation at the top.

	Barom.	Therm. attached.	Therm. detached.
	In.		
	24.1780	57°.2	56.0
Correct for the diff. of the two attached therm. viz. 4° 6, —— }	+0112		63.9
			2)119.9(59.95 mean heat.
Height of the uppermoſt barom. reduced to the fame heat as the lowermoſt, namely 61°.8, }	24.1892		31.24 ſtandard temp.
			28.71 difference.

		Feet.	Diff.
Tabular number, correſponding to, }	24.1000	7388.0	107.9
The fame, fee tab. III.	800	86.0 ⎱	
	90	9.7 ⎰ −95 9	
	2	.2	
	24.1892	7292.1	
Tabular number, correſponding to, }	28.1000	3386.6	92.6
The fame, fee tab. III.	300	28.0 ⎱	
	10	0.9 ⎰ −29.6	
	8	0.7	
	28.1318	3357.0	
And 3357.0 feet taken from ——		7292.1	
Leaves the approximate height in feet,		3935.1	
Correction for 28°7 of heat on 3935 ft.		+274.3	
Correct height of this mountain,		4209.4	

Correct for 28°.7, fee tab. IV.

28° on {	3000 =	204.1
	900 =	61.2
	35 =	2.4
0.7 on {	3000 =	5.1
	900 =	1.5
	35 =	0.0
28.7 on	3935	274 3

This

This laft obfervation was actually made, and the height geometrically was determined to be 4211.3 feet, not quite two feet different. In this example it will be obferved, that as the height of the barometer is fet down to four places of decimals; the tabular numbers, anfwering to every tenth only, are corrected by means of table III. of proportional parts, for the remaining decimals 8, 9, and 2, in one place; and 3, 1, 8, in the other; and their fum is fubducted from the numbers found in table II. And laftly, that in finding the correction for 28°.7 of heat, the fraction $\frac{7}{10}$ is confidered as fo many units, and another decimal is ftruck off; thus the correction on 3000 feet for 7° is 51; but for $\frac{7}{10}$ it becomes 5. 1, and fo of the reft.

E X A M P L E III.

In the upper gallery of the dome of St. Peter's church at Rome, and 50 feet below the top of the crofs, I obferved the barometer, from a mean of feveral obfervations, 29.5218; the thermometer attached being at 56°.6, and the detached one at 57°; at the fame time that another, placed on the banks of the Tyber one foot above the furface of the water, ftood at 30.0168, the attached thermometer at 60°.6, and the detached one at 60°.2; what was the total height of this building above the level of the river?

Observation below, at one foot above the Tyber.

Barometer.	Therm. attached.	Therm. detached.
In.		
30.0118	60.6	60°.2
	56.6	

Difference of the two attached thermometers, 4.0

Observation above, in the gallery of St. Peter's church.

	29.5218	56.6	57.0
Correct for the diff. of the two attached therm.	+ 120		60.2
			2)117.2(58.60 mean heat.
			31.24 standard temp.
Height of the uppermost barom. reduced to the heat of the lowermost *viz.* 60.5,	29.5338		27.36 difference.

		Feet.	Diff.
Tabular numbers corresponding to, —	29.5000	2119.7	88.2
	300	26.4	
	30	2.6 } —29.7	
	8	7	
	29.5338	2090.0	
Tabular numbers corresponding to, —	30.0000	1681.7	86.7
	100	8.7	
	60	5.2 } —14.6	
	8	7	
	30.0168	1667.1	
		2090.0	

Approximate height,	422.9
Correction for 27°4 of heat on 422 feet,	+28.0
Difference of height of the barometers,	450.9
Lowest barom. stood 1 foot above the river,	+1.0
Top of the cross above the gallery was,	+50.0
Total height of the top of the cross above the river Tyber, —	501.9
The same measured the same day geometrically was, — —	502.2

Correction for 27°.4
27° on { 400 = 26.2
         22 = 1.4
0.4 on 400 = .4
27.4 on 422 = 28.0

When

When the difference of the heights of the quickfilver in the two barometers happens not to exceed $\frac{1}{10}$ or even $\frac{2}{10}$ of an inch (and this will frequently be the cafe in levelling flat countries, or meafuring fmall heights) in fuch circumftances the moft convenient way of reducing the obfervations will be by means of the column of differences only; thofe numbers expreffing the length of a column of the atmofphere which correfponds to $\frac{1}{10}$ of an inch of quickfilver, at any affigned height of the barometer.

E X A M P L E IV.

Suppofe the following obfervations had been made at the top and bottom of any eminence; *viz.* at the top, barometer 29.985 inches, attached thermometer 70°5, detached thermometer 76°; and below, barometer at 30.082, attached thermometer 71°, and the detached one 68°; what was the height of the eminence?

Observation below.

Barometer.	Therm attached.	Therm. detached.
In. 30.0820	$\overset{\circ}{71}$ 0 70.5	68.0

Difference of the two attached therm.   0 5

Observation at the top.

Barometer.	Therm. attached.	Therm. detached.
In. 29 9850	$\overset{\circ}{70}.5$	$\overset{\circ}{76}.0$ 68.0

Correct for 0°.5 of heat, $+.0015$

Take — — 29.9865		2)144.0(72.0 mean heat.
From — — 30.0820		31.2 standard temp.

Remains the difference or fall of quickfilver in } 0 0955 the barometer, — }

$+40.8$ difference.

The difference for $\frac{1}{10}$ at 30 inches = 86.7 feet.

	Feet.
Therefore, for 0900 — —	78.0
0050 — —	4.3
0005 — —	0.4

Correction for 41°.

	Feet.	Ft.
41° on {	80 = 8.0	
{	2.7 = .3	
41° on	82.7 = 8.3	

Therefore, 0955 inch of quickfilver, —   82.7 the approximate height.
Correction for 41° on 82.7 feet, $+8.3$

Gives — — —   91.0 = the true height.

Now this was the height of the Tarpeian rock, or the weft-end of the Capitol-hill in Rome, above the convent of St. Clare, in the *Strada dei fpecchi.*

The preceding rules for determining heights above the furface of the earth will, I prefume, anfwer equally well for meafuring depths below it.

TABLE I. For the expansion of quicksilver by heat,

Degr. of the Therm.	Height of the barometer in inches.												
	20	21	22	23	24	25	26	27	28	29	30	31	32
1	2.0	2.1	2.2	2.3	2.4	2.5	2.6	2.7	2.8	2.9	3.0	3.1	3.2
2	4.1	4.3	4.5	4.7	4.9	5.1	5.3	5.5	5.7	5.9	6.1	6.3	6.5
3	6.1	6.4	6.7	7.0	7.3	7.6	7.9	8.2	8.5	8.8	9.1	9.4	9.7
4	8.1	8.5	8.9	9.3	9.7	10.1	10.5	11.0	11.4	11.8	12.2	12.6	13.0
5	10.1	10.6	11.1	11.6	12.1	12.7	13.2	13.7	14.2	14.7	15.2	15.7	16.2
6	12.2	12.8	13.4	14.0	14.6	15.2	15.8	16.4	17.0	17.6	18.2	18.8	19.5
7	14.2	14.9	15.6	16.3	17.0	17.7	18.4	19.2	19.8	20.6	21.3	22.0	22.7
8	16.2	17.0	17.8	18.6	19.4	20.2	21.0	21.9	22.7	23.5	24.3	25.2	25.9
9	18.2	19.2	20.1	21.0	21.9	22.8	23.7	24.6	25.6	26.5	27.4	28.3	29.2
10	20.3	21.3	22.3	23.3	24.3	25.3	26.3	27.4	28.4	29.4	30.4	31.4	32.2
11	22.3	23.4	24.5	25.6	26.7	27.8	28.9	30.1	31.2	32.3	33.4	34.5	35.6
12	24.3	25.6	26.8	28.0	29.2	30.4	31.6	32.9	34.1	35.3	36.5	37.6	38.9
13	26.3	27.7	29.0	30.3	31.6	32.9	34.2	35.6	36.9	38.2	39.5	40.8	42.1
14	28.4	29.8	31.2	32.6	34.0	35.4	36.8	38.4	39.8	41.2	42.6	43.9	45.4
15	30.4	31.9	33.4	34.9	36.4	37.9	39.4	41.1	42.6	44.1	45.6	47.1	48.6
16	32.4	34.1	35.6	37.2	38.8	40.5	42.0	43.8	45.4	47.0	48.6	50.3	51.8
17	34.5	36.2	37.9	39.6	41.3	43.0	44.7	46.6	48.3	50.0	51.7	53.4	55.1
18	36.5	38.3	40.1	41.9	43.7	45.5	47.3	49.3	51.1	52.9	54.7	56.5	58.3
19	38.5	40.5	42.3	44.2	46.1	48.1	49.9	52.1	54.0	55.9	57.8	59.7	61.6
20	40.6	42.6	44.6	46.6	48.6	50.6	52.6	54.8	56.8	58.8	60.8	62.8	64.9
21	42.6	44.7	46.8	48.9	51.0	53.2	55.2	57.5	59.6	61.7	63.8	65.9	68.1
22	44.6	46.9	49.1	51.3	53.5	55.7	57.9	60.3	62.5	64.7	66.9	69.0	71.4
23	46.6	49.0	51.3	53.6	55.9	58.2	60.5	63.0	65.3	67.6	69.9	72.2	74.6
24	48.6	51.1	53.5	55.9	58.3	60.8	63.1	65.8	68.2	70.6	73.0	75.4	77.8
25	50.7	53.2	55.8	58.2	60.7	63.2	65.7	68.5	71.0	73.5	76.0	78.5	81.1
26	52.7	55.4	58.0	60.5	63.1	65.8	68.3	71.2	73.8	76.4	79.0	81.6	84.3
27	54.7	57.5	60.3	62.9	65.6	68.3	71.0	74.0	76.7	79.4	82.1	84.8	87.5
28	56.8	59.6	62.5	65.2	68.0	70.8	73.6	76.7	79.5	82.3	85.1	87.9	90.7
29	58.8	61.8	64.7	67.5	70.4	73.3	76.2	79.5	82.4	85.3	88.2	91.1	94.1
30	60.8	63.9	66.9	69.9	72.8	75.9	78.9	82.2	85.2	88.2	91.2	94.1	97.3
31	62.8	66.0	69.1	72.2	75.2	78.4	81.5	84.9	88.0	91.1	94.2	97.4	100.5
32	64.8	68.2	71.4	74.6	77.7	81.0	84.2	87.7	90.9	94.1	97.3	100.5	103.8
33	66.9	70.3	73.6	76.9	80.1	83.5	86.8	90.4	93.7	97.0	100.3	103.0	107.0
34	68.9	72.4	75.8	79.2	82.5	86.1	89.4	93.2	96.6	100.0	103.4	106.7	110.3
35	70.9	74.5	78.0	81.5	84.0	88.6	92.0	95.9	99.4	102.9	106.4	109.9	113.5
36	73.0	76.7	80.2	83.8	86.4	91.1	94.6	98.6	102.2	105.8	109.4	113.1	116.8
37	75.0	78.8	82.5	86.2	88.9	93.6	97.3	101.4	105.1	108.8	112.5	116.2	120.0
38	77.0	80.9	84.7	88.5	91.3	96.2	99.9	104.1	107.9	111.7	115.5	119.3	123.2
39	79.0	83.1	86.9	90.8	93.7	98.7	102.5	106.9	110.8	114.7	118.6	122.5	126.5
40	81.1	85.2	89.2	93.2	97.2	101.2	105.2	109.6	113.6	117.6	121.6	125.6	129.7

TABLE II[(f)]. Giving the approximate height in Englifh feet, adapted to the temperature $31°24$ of FAHRENHEIT's thermometer.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
1.—	90309.0		16.10	19570.4	173.1	16.60	17102.5	157.5
		18062			172.0			156.5
2.—	72247.2		20	19398.4		70	16946.0	
		10565			170.9			155.6
3.—	61681.8		30	19227.5		80	16790.4	
		7496			169.8			154.6
4.—	54185.4		40	19057.7		90	16635.8	
		5814			168.6			153.7
5.—	48370.8		50	18889.1		17 00	16482.1	
		4761			167.6			152.9
6.—	43619.9		60	18721.5		10	16329.2	
		4017			166.5			151.9
7.—	39603.1		70	18555.0		20	16177.3	
		3480			165.4			151.1
8.—	36123.6		80	18389.6		30	16026.2	
		3069			164.4			150.2
9.—	33054.4		90	18225.2		40	15876.0	
		2745			163.4			149.3
10.—	30309.0		16.00	18061.8		50	15726.7	
		2484			162.4			148.5
11.—	27825.4		10	17899.4		60	15578.2	
		2267			161.3			147.6
12.—	25558.1		20	17738.1		70	15430.6	
		2086			160.4			146.8
13.—	23472.4		30	17577.7		80	15283.8	
		1931			159.3			146.0
14.—	21541.3		40	17418.4		90	15137.8	
		1798			158.4			145.2
15.00	19743.5		50	17260.0		18.00	14992.6	

(*f*) This table bears fome analogy to the tables of logiftical logarithms, being nothing more than the differences of the logarithms of the height of the barometer from the logarithm of 32 inches multiplied by fix. I have chofen the logarithm of 32 for my term of comparifon, that being the greateft probable height that the barometer will ever be feen at, even at the bottom of the deepeft mines. Had I taken the mean height of the quickfilver at the level of the fea, it is true the numbers in the table would have more truly reprefented the heights in the atmofphere, correfponding to the given height of the quickfilver; but then, in computing fmall depths or heights from the furface of the fea, we fhould have been obliged fometimes to have changed the figns in the operation, which appeared to me lefs convenient. The mean height of the barometer at the level of the fea, from 132 obfervations in Italy and in England, is 30.04 inches, the heat of the barometer being 55°, and the air 62°; fo that the term of comparifon in this table, *viz.* 32 inches, correfponds to an imaginary point within the earth at 1647 feet below the furface of the fea.

TABLE

TABLE II. continued.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
18.10	14848.3	144.3	22.00	9763.6	118.8	25.90	5511.0	100.8
20	14704.7	143.6	10	9645.5	118.1	26.00	5410.4	100.6
30	14561.9	142.8	20	9527.8	117.7	10	5310.6	99.8
40	14419 9	142.0	30	9410.7	117.1	20	5210.9	99.7
50	14276.7	141.2	40	9294.1	116.6	30	5111.6	99.3
60	14138.2	140.5	50	9178.1	116.0	40	5012.8	98.8
70	13998.5	139.7	60	9062.5	115.6	50	4914.2	98.6
80	13859.5	139.0	70	8947.4	115.1	60	4816.1	98.1
90	13721.3	138.2	80	8832 9	114 5	70	4718.3	97 8
19.00	13583.8	137.5	90	8718.9	114.0	80	4620.9	97 4
10	13447.0	136.8	23.00	8605.3	113.6	90	4523.9	97.0
20	13310.9	136.1	10	8492 3	113.0	27.00	4427.2	96.7
30	13175.6	135.3	20	8379.7	112.6	10	4330.8	96.4
40	13041.1	134.5	30	8267.6	112.1	20	4234.9	95.9
50	12906.9	134.2	40	8156.0	111.6	30	4139.2	95.7
60	12773.6	133.3	50	8044.9	111.1	40	4044.0	95.2
70	12641.0	132.6	60	7934.3	110.6	50	3949.0	95.0
80	12509.1	131.9	70	7824.1	110.2	60	3854.5	94.5
90	12337.0	131.3	80	7714.4	109.7	70	3760.2	94.3
20.00	12247.2	130.6	90	7605.1	109.3	80	3666.3	93 9
10	12117.2	130.0	24.00	7496.3	108.8	90	3572.7	93.6
20	11987.9	129.3	10	7388.0	108.3	28.00	3479.5	93.2
30	11859.2	128.7	20	7280.1	107.9	10	3386.6	92 9
40	11731.2	128.0	30	7172.6	107.5	20	3294.0	92.6
50	11603.8	127.4	40	7065.6	107.0	30	3201.8	92 2
60	11477.0	126.8	50	6959.0	106.6	40	3109.9	91.9
70	11350.0	126.2	60	6852.9	106.1	50	3018.3	91.6
80	11225.2	125.6	70	6747.2	105.7	60	2927.0	91.3
90	11100.2	125.0	80	6641.9	105.3	70	2836.1	90.9
21.00	10975.8	124.4	90	6537.0	104.9	80	2745.4	90.7
10	10852.1	123.7	25.00	6432.6	104.4	90	2655.1	90.3
20	10728.8	123.3	10	6328.6	104.0	29.00	2565.1	90.0
30	10606.2	122.6	20	6225.0	103 6	10	2475.4	89.7
40	10484.2	122.0	30	6121.8	103.2	20	2386.0	89.4
50	10362.7	121.5	40	6019.0	102.8	30	2296.9	89.1
60	10241.8	120.9	50	5916.6	102.4	40	2208.2	88.7
70	10121.4	120.4	60	5814.6	102.0	50	2119.7	88.5
80	10001.6	119 8	70	5713.0	101.6	60	2031.5	88 2
90	9882.4	119.2	80	5611.8	101.2	70	1943.6	87.9

T A B L E II. continued.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
29.80	1856.0	87.6	30.60	1165.7	85.3	31.40	493.2	83.1
90	1768.7	87.3	70	1080.7	85.0	50	410.4	82.8
30.00	1681.7	87.0	80	996.0	84.7	60	327.8	82.6
10	1595.0	86.7	90	911.5	84.5	70	245.4	82.4
20	1508.6	86.4	31.00	827.3	84.2	80	163.4	82.0
30	1422.4	86.2	10	743.4	83.9	90	81.6	81.0
40	1236.6	85.8	20	659.7	83.7	32.00	00.0	81.6
50	1251.0	85.6	30	576.3	83.4			

TABLE III. Of proportional parts.

Diff.	1	2	3	4	5	6	7	8	9	Diff.	1	2	3	4	5	6	7	8	9
81	8	16	24	32	40	49	57	65	73	106	11	21	32	42	53	64	74	85	95
82	8	16	25	33	41	49	57	66	74	107	11	21	32	43	53	64	75	86	96
83	8	17	25	33	41	50	58	66	75	108	11	22	3.	43	54	65	76	86	97
84	8	17	25	34	42	50	59	67	76	109	11	22	33	44	54	65	76	87	98
85	8	17	25	34	42	51	59	68	76	110	11	22	33	44	55	66	77	88	99
86	9	17	26	34	43	52	60	69	77	111	11	22	33	44	55	67	78	89	100
87	9	17	26	35	43	52	61	70	78	112	11	22	34	45	56	67	78	90	101
88	9	18	26	35	44	53	62	70	79	113	11	23	34	45	55	68	79	90	102
89	9	18	27	36	44	53	62	71	80	114	11	23	34	46	57	68	80	91	103
90	9	18	27	36	45	54	63	72	81	115	11	23	34	46	57	69	80	92	103
91	9	18	27	36	45	55	64	73	82	116	12	23	35	46	58	70	81	93	104
92	9	18	28	37	46	55	64	74	83	117	12	23	35	47	58	70	82	94	105
93	9	19	28	37	46	56	65	74	84	118	12	24	35	47	59	71	83	94	106
94	9	19	28	38	47	56	66	75	85	119	12	24	36	48	59	71	83	95	107
95	9	19	28	38	47	57	66	76	85	120	12	24	36	48	60	72	84	96	108
96	10	19	29	38	48	58	67	77	86	121	12	24	36	48	60	73	85	97	109
97	10	19	29	39	48	58	68	78	87	122	12	24	37	49	61	73	85	98	110
98	10	20	29	39	49	59	69	78	88	123	12	25	37	49	61	74	86	98	111
99	10	20	30	40	49	59	69	79	89	124	12	25	37	50	62	74	87	99	112
100	10	20	30	40	50	60	70	80	90	125	12	25	37	50	62	75	87	100	112
101	10	20	30	40	50	61	71	81	91	126	13	25	38	50	63	76	88	101	113
102	10	20	31	41	51	61	71	82	92	127	13	25	38	51	63	76	89	102	114
103	10	21	31	41	51	62	72	82	93	128	13	26	38	51	64	77	90	102	115
104	10	21	31	42	52	62	73	83	94	129	13	26	39	52	64	77	90	103	116
105	10	21	31	42	52	63	73	84	94	130	13	26	39	52	65	78	91	104	117

TABLE IV. For the expanſion of the air, or correc
tion of the uppermoſt height, ſee p. 576.

Deg.	Approximate height in feet.								
o	1000.	2000.	3000.	4000.	5000.	6000.	7000.	8000.	9000.
1	2.4	4.9	7.3	9.7	12 1	14 6	17.0	19.4	21.9
2	4.9	9.7	14.6	19 4	24.3	29 2	34 0	38.9	43 7
3	7.3	14.6	21.9	29.2	36 4	43.7	51.0	58.3	65 6
4	9.7	19.4	29.2	38.9	48.6	58 3	68.0	77.8	87.5
5	12.1	24.3	36.4	48.6	60.7	72.9	85.0	97.2	109.3
6	14.6	29.2	43.7	58.3	72.8	87.5	102.0	116.6	131.2
7	17.0	34.0	51.0	68.0	85.0	102.1	119.0	136.1	153.0
8	19.4	38.9	58.3	77.8	97.1	116.6	136.0	155.5	174.9
9	21.9	43.7	65.6	87.5	109.3	131.2	153.0	175.0	196.8
10	24.3	48.6	72.9	97.2	121.5	145 8	170.1	194 4	218.7
11	26.7	53.5	80.2	106.9	133 6	160.4	187.1	213 8	240.6
12	29.2	58.3	87.5	116 6	145.8	175.0	204.1	233.3	262 4
13	31.6	63 2	94.8	126.4	157.9	189.5	221.1	252.7	284.3
14	34 0	68.0	102.1	136.1	170.1	204.1	238.1	272.2	306.2
15	36.4	72.9	109.3	145.8	182.2	218.7	255.1	291.6	328.0
16	38.8	77.8	116.6	155.5	194.3	233.3	272.1	311,0	349.9
17	41.3	82.6	123.9	165.2	206.5	247.9	289.1	330.5	371.7
18	43.7	87.5	131.2	175.0	218.6	262.4	306.1	349 9	393.6
19	46.1	92 3	138.5	184.7	230.8	277.0	323.1	369.4	415.5
20	48.5	97.2	145.8	194 4	243 0	291.6	340.2	388 8	437.4
21	51.0	102.1	153.1	204.1	255.1	306.2	357.2	408.2	459.3
22	53.5	106 9	160.4	213.8	267.3	320.8	374 2	427.7	481.1
23	55.9	111 8	167.7	223.6	279.4	335.3	391.2	447 1	503.0
24	58.3	116.6	175.0	233.3	291 6	349 9	408.2	466.6	524.9
25	60.7	121.5	182.2	243 0	303.7	364 5	425 2	486 0	546.7

T

TABLE

TABLE IV. continued.

Deg.	Approximate height in feet.								
o	1000	2000.	3000.	4000	5000.	6000.	7000.	8000	9000.
26	63.1	126.4	189.5	252.7	315.8	379.1	442.2	505.4	568.6
27	65.6	131.2	196.8	262.4	328.0	393.7	459.2	524.9	590.4
28	68.0	136.1	204.1	272.2	340.1	408.2	476.2	544.3	612.3
29	70.4	140.9	211.4	281.9	352.3	422.8	493.2	563.8	634.2
30	72.9	145.8	218.7	291.6	364.5	437.4	510.3	583.2	656.1
31	75.3	150.7	226.0	301.3	376.6	452.0	527.3	602.6	678.0
32	77.8	155.5	233.3	311.0	388.8	466.6	544.3	622.1	699.8
33	80.2	160.4	240.6	320.8	400.9	480.1	561.3	641.5	721.7
34	82.6	165.2	247.9	330.5	413.1	495.7	578.3	661.0	743.6
35	85.0	170.1	255.1	340.2	425.2	510.2	595.3	680.4	765.4
36	87.4	175.0	262.4	349.9	437.3	524.8	612.3	699.8	787.3
37	89.9	179.8	269.7	359.6	449.5	539.4	629.3	719.3	809.1
38	92.3	184.7	277.0	369.4	461.6	553.9	646.3	738.7	831.0
39	94.7	189.5	284.3	379.1	473.8	568.5	663.3	758.2	852.9
40	97.2	194.4	291.6	388.8	486.0	583.2	680.4	777.6	874.8
41	99.6	199.3	298.9	398.5	498.1	597.8	697.4	797.0	895.7
42	102.1	204.1	306.2	408.2	510.3	612.4	714.4	816.5	918.5
43	104.5	209.0	313.5	418.0	522.4	626.9	731.4	835.9	940.4
44	106.9	213.8	320.8	427.7	534.6	641.5	748.4	855.4	962.3
45	109.3	218.7	328.0	437.4	546.7	656.1	765.4	874.8	984.1
46	111.7	223.6	335.3	447.1	558.8	670.7	782.4	894.2	1006.0
47	114.2	228.4	342.6	456.8	571.0	685.3	799.4	913.7	1027.8
48	116.6	233.3	349.9	466.6	583.1	699.8	816.4	933.1	1049.7
49	119.0	238.1	357.2	476.3	595.3	714.4	833.4	952.6	1071.6
50	121.5	243.0	364.5	486.0	607.5	729.0	850.5	972.0	1093.5

Table

Table of heights taken by the barometer, &c.

	+ or — the Lake of Geneva.	Above the Mediterranean.
	Feet.	Feet.
The Lake of Geneva, from 18 obſervations, —	0	1230 (g)
Greateſt depth of the Lake, — —	— 393	
Cluſe, at the Croix Blanche, firſt-floor, (h) 2,	+ 351	1581
Chamouny, ground-floor of the inn near the foot of } Mont Blanc, 4 — — —	+ 2137	3367
The Montanvert, at the Chateâu, 1 —	+ 5001	6231
The ſource of the river Arvéron, at the bottom of the } Vallée de Glace, 1 — — —	+ 2426	3656
Salenche, at the inn, ſecond-floor, 1 —	+ 664	1941
La Bonne-Ville, a la Ville de Geneve, ſecond floor, 1	+ 245	1475
Chatlaino, country houſe near Geneva, g ound-floor, G	+ 178	
The ball on the higheſt, or ſouth-weſt, tower of St. } Peter's church in Geneva, G — —	+ 249	
St. Joire, in a field at the foot of the Mole, G —	+ 671	1901
Summit of the Mole, — — — —	+ 4883	6113
Pitton, higheſt point of Mont Saleve, G —	3284	4514
The Dole, higheſt ſummit of Mont Jura, G —	+ 4293	5523
The Buet, G — — — — —	+ 8894	10124
Aiguille d'Argentiére, G — — —	+12172	13402
Mont Blanc, G — — — —	+14432	15662
Frangy, at the inn, firſt-floor, below the Lake,	— 166	
Aix, a la Ville de Geneve, firſt-floor, below the Lake,	— 378	
Chambery, au St. Jean Baptiſte, firſt-floor, below the Lake,	— 352	
Aiguebelle, at the inn, firſt-floor, below the Lake,	— 190	
La Chambre, at the inn, firſt-floor, above the Lake,	+ 337	
St. Michael, at the inn, firſt-floor, —	+ 1113	2343
Modane, at the inn, firſt floor, — —	+ 2220	3450

(g) More correctly 1228 feet, but I have taken it at 1230 in round numbers.

(h) The figures at the end of ſome of the names ſhew the number of obſervations that were made; and the letter G indicates ſuch obſervations to have been geometrical.

Table

Table of heights, &c. continued.

	+ or — the Lake of Geneva.	Above the Mediterranean.
	Feet.	Feet.
Lannebourg, the foot of Mont Cenis, at the inn, first-floor, +	3178	4408
Mont Cenis, at the Post , — — — +	5031	6261
———— at the Grande Croix, — — +	4793	6023
Novalese, the foot of Mont Cenis on the side of Italy, at the inn, first-floor, — — } +	1511	2741
Boucholin, on the first-floor, — — +	213	
St. Ambroise, on the first-floor, below the Lake, · —	40	
Turin, a l'Hôtel d'Angleterre, second-floor, 4 —	289	941
Felissano, near Alessandria, first-floor, 1 — —	671	
Piacenza, St. Marco, first-floor, 1 — — —	967	263
Parma, au Paon, first-floor, 3 — — —	923	307
Bologna, au Pelerin, first-floor, 3 — —	831	399
Loiano, a little village on the Appenines, between Bologna and Florence, — — }		2591
The mountain Raticosa, the highest point of the Appenines the road passes over, 1¼ miles beyond Filecaije in going to Covigliaje, — — } +	1671	2901
Florence, nel Corso dei Tintori, 50 feet above the Arno, which was 18 feet below the wall of the quay, 3 } —	990	+240
Pisa, aux Trois Demoiselles, second-floor, 4 — · —	1228	+54½
Leghorn, chez Muston, second-floor, 2 — —	1244	+38
Siena, aux Trois Rois, second-floor, 2 — —	164	1066
Redicoffani, at the Post, first-floor, above the Lake, +	1240	2470
———— the top of the tower of the old fortification on the summit of the rock, — }	1830	3060
Viterbo, aux Trois Rois, first-floor, on the Ciminus of the Ancients, — — — } +	29	1259
Rome, nel Corso, 61 feet above the Tyber, 7 —	1084	94

(i) The rocks on each side the plain, where the post-house stands, are at least 3000 feet higher than this situation; and it is from the snow on the tops, and through the crevices, that the lake on this plain is formed, which gives rise to the Dora, and may be called one of the sources of the Po.

Table of heights, &c. continued.

	Above the River Tyber. Feet.	Above the Mediterranean. Feet.
The Level of the river Tyber, — —		33
The top of the Janiculum, near the Villa Spada, —	260	
Aventine Hill, near the Priory of Malta, —	117	
In the Forum, near the arch of SEVERUS, where the ground is raiſed 23½ feet, — —	34	
Palatine Hill, on the floor of the Imperial palace —	133	
Celian Hill, near the CLAUDIAN aqueduct, —	125	
Bottom of the canal of the CLAUDIAN aqueduct, —	175	
Eſquiline Hill, on the floor of St. M. Major's church,	154	
Capitol Hill, on the Weſt-end of the Tarpeian rock,	118	
In the Strada dei Specchi, in the convent of St. Clare,	27	
On the union of the Viminal and Quirinal Hills, in the Carthuſian's church, DIOCLES. Baths, —	141	
Pincian Hill, in the garden of the Villa Medici, —	165	
Top of the croſs of St. Peter's church, —	502	
The baſe of the obeliſk, in the center of the Periſtyle,	31	
The ſummit of the mountain Soracte, lying about 20½ geog. miles N. of Rome, G — —		2271
The ſummit of Monte Velino, one of the Appenines, covered with ſnow in June, about 46 geog. miles N.W. of Rome, and which is probably the higheſt of the Appenines, G — —		8397

	+ or — the Lake of Geneve.	
Naples, Caſa Iſolata on the Chiaia, 27½ feet above the ſea, 5 — — —	— 1197	
Mount Veſuvius, mouth of the Crater from whence the fire iſſued in 1776, — —		3938 (k)

Table

(k) Sir WILLIAM HAMILTON informed me, that the height of Veſuvius, as taken by Mr. DE SAUSSURE of Geneva in 1772, with only a barometer of Mr. DE LUC's conſtruction, and according to his rules, was 3659½ French feet = 3900 Engliſh, which agrees pretty well with mine. But the Padre DELLA TORRE pretends to have found the height of Veſuvius in 1752 (ſee p. 44. of

his

Table of heignts, &c. continued.

	+ or — the Lake of Geneva.	Above the Mediterranean.
Mount Vesuvius, at the base of the cone,	— — —	2021
Top of the mountain Somma,	— —	3738
The summit of Mount Ætna,	— —	10954 (1)

The following heights are determined from corresponding observations by Mr. MESSIER at Paris, whose barometer is supposed 108 feet above the sea.

Barberino di Valdensa, between Boggebonri and Tavernelle,	974
Modena, a l'Albergo nuovo, — —	214
Montmelian, at 20 feet above the river, — —	811
Monte Viso, by an observation from Jurin, by means accurate, G — — — }	9997
Monte Rosa, as measured geometrically by the Father BECCARIA, being the second mountain of all the Alps, — — — — }	15084
Pont Beauvoisin, — — — —	705
La tour du Pin, 4 — — — —	938
Verpilliére, — — — —	566

his History of this Mountain) = 1677 French feet only, the difference of his barometer at the top and at the level of the sea being no more than 23¼ French lines = 2.065 English inches, which was certainly a mistake of little less than 2000 feet in the result. The Abbé NOLLET in 1749 found the fall of the quicksilver 40 lines = 3.55 inches English; and, if this observation is to be depended upon, the summit of this volcano has risen within these 27 years more than 330 feet perpendicular.

(1) I have ventured to compute the height of this celebrated mountain from my own tables, though from an observation of Mr. DE SAUSSURE's in 1773, which that gentleman obligingly communicated to me. It will serve to shew, that this volcano is by no means the highest mountain of the old world; and that Vesuvius, placed upon Mount Ætna, would not be equal to the height of Mont Blanc, which latter I take to be the most elevated point in Europe, Asia, or Africa.

The circumference of the visible horizon on the top of Mount Ætna, allowance being made for refraction, which I estimate at 6', is 1093 English miles.

Table

Table of heights, &c. continued.

	+ or — the Lake of Geneva.	Above the Mediterranean.
Lyons, at the Hôtel Blanc, 50 feet above the Saône,		449
St. Jean le vieux, — — —		695
Cerdon, near the poſt-houſe at the foot of the rocks,		854
Nantua, 10 feet above the Lake, — —		1423
Chatillon, at the Logis Neuf, — —		1629
Colonges, — — — —		1626
St. Genis, apparently on a level with the foot of Mont Jura,		1501
Geneva, at 100 feet above the Lake, 5 —		1268 *(m)*
Mâcon, at the Parc, 24 feet above the Saône —		514
Dijon, à la Cloche, the firſt-floor, — —		710
Mountain of Maraiſelois *(n)*, 4¾ miles beyond Viteaux towards Dijon, — —		1677
Lucy-le-bois, — — — —		645
Auxerre, 50 feet above the river, — —		283
Sens, at the Poſt, — — —		163
Fontainbleau, at the Grand Cerf, ſecond-floor, —		242

(m) From this compariſon with Mr. MESSIER's obſervations at Paris, which makes the Lake of Geneva only 1168 feet above the level of the ſea (whereas from 18 obſervations in Italy, near the ſhore of the Mediterranean, it appears to be 1228, *viz.* +60 feet different) I am inclined to believe, that Mr. MESSIER's place of obſervation is about 50 feet higher than I have ſuppoſed it, *viz.* 160 feet above the ſea inſtead of 108, as deduced from three obſervations only at Boulogne, Calais, and at Dover. If this be allowed, the ſame number of feet muſt be added alſo to all the other heights that are determined by compariſon with Mr. MESSIER's obſervations. I am, however, by no means ſure of this, but leave it to future obſervers.

(n) On one ſide of this mountain is a little ſtream called Amançon, that joins the Yonne and the Seine, and thus goes to the Atlantic; while on the other ſide is found the Ouche, which, uniting with the Saône and the Rhone, runs to the Mediterranean: this part of Burgundy then ſeems to be one of the higheſt in France.

5   Table

Table of heights, &c. continued.

	$+$ or $-$ the Seine at Paris.	Above the Mediterranean.
Paris, mean height of the Seine, - that is, *quand les eaux fe trouvent à* 13 *pieds* 9 *pouces fur l'echelle du Pont Royal felon M.* DE LA LANDE, —		$36\frac{1}{2}$
Place of my own obfervations in the Ruë Jacob, fecond-floor, — — —	$+ 57$	
Mr. MESSIER's obfervatory, at the Hôtel de Clugny, firft floor, · — — —	72	
Mr. DE LA LANDE's ditto, at the College Royal, firft-floor,	101	
Place of Monf. le Pere COTTES's obfervations at Montmorency, 10 miles North of Paris, —	333·	
Stone-gallery of the Church on Mont Valerien,	473	
Depth of the cave of the Royal Obfervatory at Paris below the pavement, — —	$98\frac{1}{2}$	
The fame, according to Mr. DE LA LANDE, by actual meafurement, — — —	98·	
Height of the north tower of the church of Notre Dame above the floor, — —	$220\frac{1}{2}$	
———— by actual meafurement, — —	$218\frac{1}{2}$	
Chantilly, · — — — —		119
Clermont, — — — —		329
Amiens, Ruë de Noyon, firft-floor, — —		147
Abbeville, firft-floor, — — —		79

	Below the mean height of the Seine.	
Boulogne, mean level of the fea, from one obferv. only,	-33.9	
Calais, ditto, from one obfervation, — —	-38.8	
Dover, ditto, from three obfervations made two years preceding thofe at Calais and Boulogne, —	-36.6	
Mean height of the river (*o*) Thames at London above the mean height of the river Seine from five direct comparifons with Mr. MESSIER, —	$+ 6.8$	
And confequently the Thames at London above the fea,		43
Warwick, mean level of the river Avon, . —		155
Shuckburgh-houfe, in Warwickfhire, — —		560

(*o*) By the mean height of the river Thames is underftood when the water is $15\frac{1}{2}$ feet below the pavement in the left-hand arcade at Buckingham-ftairs.

XXX. *An Account of the* Bramin's *Observatory at* Benares. *By Sir* Robert Barker, *Knt. F. R. S.; in a Letter to Sir* John Pringle, *Bart. P. R. S.*

S I R,

Read May 29, 1777.

BENARES in the Eaſt Indies, one of the principal ſeminaries of the Bramins or prieſts of the original Gentoos of Hindoſtan, continues ſtill to be the place of reſort of that ſect of people; and there are many publick charities, hoſpitals, and pagodas, where ſome thouſands of them now reſide. Having frequently heard that the ancient Bramins had a knowledge of aſtronomy, and being confirmed in this by their information of an approaching eclipſe both of the Sun and Moon, I made inquiry, when at that place in the year 1772, among the principal Bramins, to endeavour to get ſome information relative to the manner in which they were acquainted of an approaching eclipſe. The moſt intelligent that I could meet with, however, gave me but little ſatisfaction. I was told, that theſe matters were confined to a few, who were in poſſeſſion of certain books and records; ſome containing the myſ-

teries

terics of their religion, and others the tables of aftro-
nomical obfervations, written in the Skanfkirrit lan-
guage, which few underftood but themfelves: that they
would take me to a place which had been conftructed for
the purpofe of making fuch obfervations as I was in-
quiring after, and from whence they fuppofed the
learned Bramins made theirs. I was then conducted to an
ancient building of ftone, the lower part of which, in its
prefent fituation, was converted into a ftable for horfes,
and a receptacle for lumber; but, by the number of
court-yards and apartments, it appeared that it muft once
have been an edifice for the ufe of fome public body of
people. We entered this building, and went up a ftair-
cafe to the top of a part of it, near to the river Ganges,
that led to a large terrace, where, to my furprize and
fatisfaction, I faw a number of inftruments yet remain-
ing, in the greateft prefervation, ftupendoufly large, im-
moveable from the fpot, and built of ftone, fome of them
being upwards of twenty feet in height; and, although
they are faid to have been erected two hundred years
ago, the graduations and divifions on the feveral arcs ap-
peared as well cut, and as accurately divided, as if they
had been the performance of a modern artift. The exe-
cution in the conftruction of thefe inftruments exhibited,
a mathematical exactnefs in the fixing, bearing, and
fitting

fitting of the feveral parts, in the neceffary and fufficient fupports to the very large ftones that compofed them, and in the joining and faftening each into the other by means of lead and iron.

The fituation of the two large quadrants of the inftrument marked A in the firft plate, whofe radius is nine feet two inches, by their being at right angles with a gnomon at twenty-five degrees elevation, are thrown into fuch an oblique fituation as to render them the moft difficult, not only to conftruct of fuch a magnitude, but to fecure in their pofition for fo long a period, and affords a ftriking inftance of the ability of the architect in their conftruction; for, by the fhadow of the gnomon thrown on the quadrants, they do not appear to have altered in the leaft from their original pofition; and fo true is the line of the gnomon, that, by applying the eye to a fmall iron ring of an inch diameter at one end, the fight is carried through three others of the fame dimenfion to the extremity at the other end, diftant thirty-eight feet eight inches, without obftruction; fuch is the firmnefs and art with which this inftrument has been executed. This performance is the more wonderful and extraordinary when compared with the works of the artificers of Hindoftan at this day, who are not under the immediate

<div align="right">direction</div>

direction of an European mechanic; but arts appear to have declined equally with science in the East.

Lieutenant-colonel ARCHIBALD CAMPBELL, at that time chief engineer in the East India Company's service at Bengal, a gentleman whose abilities do honour to his profession, made a perspective drawing of the whole of the apparatus that could be brought within his eye at one view; but I lament he could not represent some very large quadrants, whose radii were about twenty feet, they being on the side from whence he took his drawing. Their description however is, that they are exact quarters of circles of different radii, the largest of which I judged to be twenty feet, constructed very exactly on the sides of stone walls built perpendicular, and situated, I suppose, in the meridian of the place: a brass pin is fixed at the center or angle of the quadrant, from whence, the Bramin informed me, they stretched a wire to the circumference when an observation was to be made; from which it occurred to me, the observer must have moved his eye up or down the circumference, by means of a ladder or some such contrivance, to raise and lower himself, until he had discovered the altitude of any of the heavenly bodies in their passage over the meridian, so expressed on the arcs of these quadrants: these arcs were very exactly divided into

nine large fections; each of which again into ten, making ninety leffer divifions or degrees; and thofe alfo into twenty, expreffing three minutes each, of about two-tenths of an inch afunder; fo that it is probable, they had fome method of dividing even thefe into more mi-nute divifions at the time of obfervation.

My time would only permit me to take down the par-ticular dimenfions of the moft capital inftrument, or the greater equinoctial Sun-dial, reprefented by figure A, plate 12. which appears to be an inftrument to exprefs folar time by the fhadow of a gnomon upon two qua-drants, one fituated to the eaft, and the other to the weft of it; and indeed the chief part of their inftruments at this place appear to be conftructed for the fame purpofe, ex-cept the quadrants, and a brafs inftrument that will be defcribed hereafter.

Figure B is another inftrument for the purpofe of de-termining the exact hour of the day by the fhadow of a gnomon, which ftands perpendicular to and in the cen-ter of a flat circular ftone, fupported in an oblique fitua-tion by means of four upright ftones and a crofs-piece; fo that the fhadow of the gnomon, which is a perpendi-cular iron rod, is thrown upon the divifions of the circle defcribed on the face of the flat, circular ftone.

5

Figure

Figure c is a braſs circle, about two feet diameter, moving vertically upon two pivots between two ſtone pillars, having an index or hand turning round horizontally on the center of this circle, which is divided into 360 parts; but there are no counter diviſions on the index to ſub-divide thoſe on the circle. This inſtrument appears to be made for taking the angle of a ſtar at ſetting or riſing, or for taking the azimuth or amplitude of the Sun at riſing or ſetting.

The uſe of the inſtrument, figure d, I was at a loſs to account for. It conſiſts of two circular walls; the outer of which is about forty feet diameter, and eight feet high; the wall within about half that height, and appears intended for a place to ſtand on to obſerve the diviſions on the upper circle of the outer wall, rather than for any other purpoſe; and yet both circles are divided into 360 degrees, each degree being ſub-divided into twenty leſſer diviſions, the ſame as the quadrants. There is a door-way to paſs into the inner circle, and a pillar in the center, of the ſame height with the lower circle, having a hole in it, being the center of both circles, and ſeems to be a ſocket for an iron rod to be placed perpendicular into it. The diviſions on theſe, as well as all the other inſtruments, will bear a nice examination with a pair of compaſſes.

4 I 2   Figure

Figure E is a fmaller equinoctial Sun-dial, conftructed upon the fame principle as the large one A.

I cannot quit this fubject without obferving, that the Bramins, without the affiftance of optical glaffes, had neverthelefs an advantage unexperienced by the obfervers of the more Northern climates. The ferenity and clearnefs of the atmofphere in the night-time in the Eaft Indies, except at the feafons of changing the monfoons or periodical winds, is difficult to exprefs to thofe who have not feen it, becaufe we have nothing in comparifon to form our ideas upon: it is clear to perfection, a total quietude fubfifts, fcarcely a cloud to be feen; and the light of the heavens, by the numerous appearance of the ftars, affords a profpect both of wonder and contemplation.

This obfervatory at Benares is faid to have been built by the order of the emperor ACKBAR; for as this wife prince endeavoured to improve the arts, fo he wifhed alfo to recover the fciences of Hindoftan, and therefore directed that three fuch places fhould be erected; one at Delhi, another at Agra, and the third at Benares.

Some doubts have arifen with regard to the certainty of the ancient Bramins having a knowledge in aftronomy, and whether the Perfians might not have introduced it into Hindoftan when conquered by that people;

but

but thefe doubts I think muft vanifh, when we know that the prefent Bramins pronounce, from the records and tables which have been handed down to them by their forefathers, the approach of the eclipfes of the Sun and Moon, and regularly as they advance give timely information to the emperor and the princes in whofe dominion they refide. There are yet fome remains in evidence of their being at one time in poffeffion of this fcience. The figns of the zodiac, in fome of their Choultrys on the coaft of Coromandel, as remarked by JOHN CALL, efq. F. R. S. in his letter to the Aftronomer Royal, requires little other confirmation. Mr. CALL fays, that as he was laying on his back, refting himfelf in the heat of the day, in a Choultry at Verdapetah in the Madura country, near Cape Commorin, he difcovered the figns of the zodiac on the cieling of the Choultry: that he found one, equally compleat, which was on the cieling of a temple, in the middle of a Tank before the pagoda Teppecolum near Mindurah; and that he had often met with feveral parts in detached pieces. See Philof. Tranf. 1772, p. 353. Thefe buildings and temples were the places of refidence and worfhip of the original Bramins, and bear the marks of great antiquity, having perhaps been built before the Perfian conqueft. Befides, when we know that the manners and cuftoms of the Gentoo

<div align="right">religion</div>

religion are fuch as to preclude them from admitting the fmalleft innovation in their inftitutions; when we alfo know that their fafhion in drefs, and the mode of their living, have not received the leaft variation from the earlieft account we have of them; it cannot be fuppofed they would engrave the fymbolical figures of the Perfian aftronomy in their facred temples; the figns of the zodiac muft therefore have originated with them, if we credit their tradition of the purity of their religion and cuftoms.

Mr. FRASER, in his Hiftory of the Mogul Emperors, fpeaking of time fays, " the Lunar year they reckon " 354 days, 22 gurris, 1 pull; the Solar year they " reckon 365 days, 15 gurris, 30 pulls, $22\frac{1}{2}$ peels; 60 " peels making 1 pull, 60 pulls 1 gurri, and 60 gurris " 1 day. This is according to the Bramins or Indian " priefts, and what the Moguls and other Mahomme- " dans in India chiefly go by."

Thus far Mr. FRASER; and it ferves to ftrengthen the argument for fuppofing that the Bramins had a knowledge of aftronomy before the introduction of Mahometanifm into Hindoftan.

Dimenfions

Dimensions of the larger equinoctial Sun-dial, plates
13. and 14.

	Feet.	In.
Length of the gnomon at the base *b b,*	34	8
Oblique length of the gnomon *c c,*	38	8
Radius of the quadrants *a a,*	9	2
Height of the gnomon at *d,*	22	3
Breadth of the quadrants *f f,*	5	10
Thickness *g g,*	1	0
Breadth of the gnomon *h h,*	4	6
Whole extent of the instrument *i i,*	37	4

Latitude of the place taken by double altitude 25° 10′.

I am, &c.

XXXI. *A short Account of Dr.* Maty's *Illness, and of the appearances in the dead Body, which was examined on the* 3d *of* July, 1776, *the Day after his Decease. By Dr.* Hunter *and Mr.* Henry Watson, *FF. R. S.*

Read May 1, 1777.

ABOUT two weeks before he died, he was taken with a fit of violent oppressive pain, juft above the pit of the ftomach, which made him feel as if he was very near dying. He was bled, and gradually recovered; yet fo imperfectly, that any motion of his body, or any preffure upon that part with the point of a finger, inftantly brought on fuch oppressive pain, that he was convinced the leaft addition to what he had feveral times felt, muft have put an end to his life. He had an idea that there might be a collection of matter behind the fternum, which might be difcharged by fome chirurgical operation.

Upon examining the part, which with the whole body was very much emaciated, there was no protrufion or difcolouration. All thoughts of making any perforation were laid afide; and it was thought probable, that there was fome inflammation or adhefion of the pericar-

dium
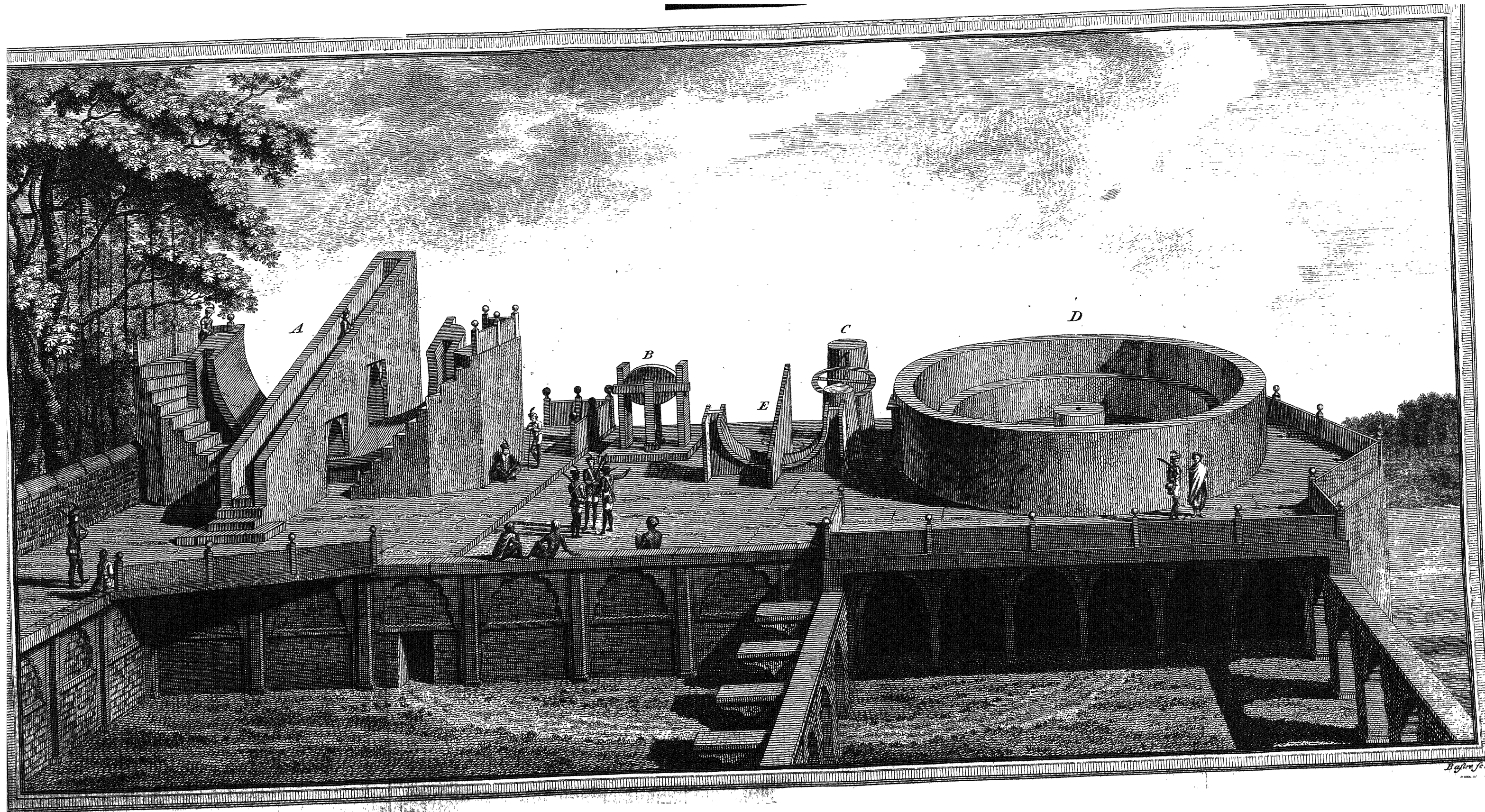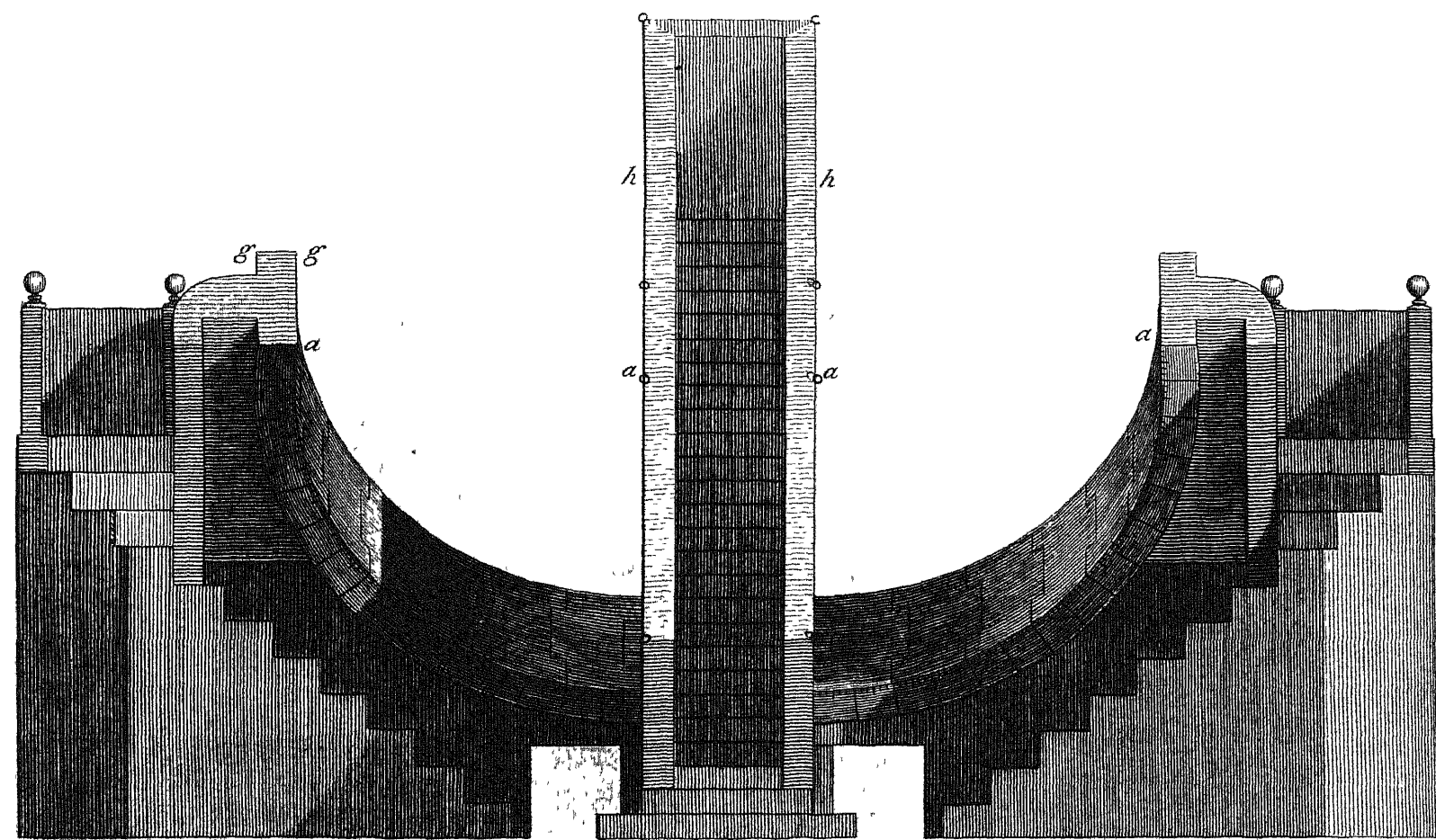
Scale of Feet.

0 1 2 3 4 5 6 7 8 9 10 11 12

Basire fc.

Scale of Feet.

dium, or of the heart itſelf, at its anterior part, juſt above the diaphragm. His cough was almoſt inceſſant in the night ſince he had left off the uſe of opium, to which he had been long accuſtomed. For ſeven or eight years, he ſaid, he believed he might have had twenty purging ſtools in every twenty-four hours, from a complaint in his bowels, the principal ſeat of which he pointed out ſo exactly in his emaciated ſtate, that it was obſerved at the time it muſt be in the colon, where it paſſes down on the outſide of the lower end of the left kidney. It was therefore thought probable that there was contraction with internal ulceration of the gut at that place: and about three years ago, with this complaint, which always continued in his bowels and left ſide, he had a fiſtula in ano, for which he was cut, and thereby cured of that diſorder; but from that time, he was always ſenſible that the lower part of the rectum remained in an awkward, uneaſy ſtate, ſo that it was difficult and painful to paſs a common gliſter-pipe into it.

His medical friends were of opinion, that no more could be done for him than to palliate, and to procure eaſe and ſleep. He returned to his opium, of which he took one grain twice a day; and at times was thereby much relieved and comforted.

The

The heart and lungs were examined with great care, but there was hardly any appearance of diforder in either, contrary to what was expected.

The conjecture that had been formed about the complaint in the bowels proved to be perfectly juft. The fmall inteftines were apparently pretty found; the cæcum and beginning of the colon were much diftended with air, but not inflamed. The arch, or tranfverfe turn of the colon, was likewife much diftended, and its blood-veffels were fo loaded, that there was, at firft fight, the outward appearance of an internal inflammation. The enlarged part of the colon terminated at the lower end of the left kidney, where there was an annular ftricture on the outfide of the gut, and there the gut felt hard and flefhy. The enlarged part being flit up, was much inflamed and fuperficially ulcerated on the infide, and more in proportion towards the lower end. At the ftricture there was but a very fmall paffage left, winding irregularly through an inch and an half of hard ulcerated gut. Below this, where the colon paffes over the pfoas and iliac veffels, it was in its natural ftate; but the rectum had been at fome former time very much difeafed, and for a finger's length to within two inches of the anus was contracted to almoft a goofe-quill fize, and of a livid colour. The lower two inches were not fo much con-

tracted,

tracted, but of the ſame livid colour, and the ſurface of the gut there was almoſt as unequal as the faſciculated ſurfaces in the heart; the effect, probably, of univerſal ulceration there, which had been a part of, or a companion to, the fiſtula, of which he had been cured by the operation; for, on that part, the villous coat of the inteſtine was deſtroyed.

TO this account, more particularly of the two laſt weeks of Dr. MATY's illneſs, and of the appearances upon opening the body, as drawn up by Dr. HUNTER, I ſhall beg leave to add the few following remarks.

The heart and lungs were indeed neither of them eſſentially diſeaſed; yet there was a whitiſh ſpot, about the breadth of a ſix-pence, upon the right ventricle of the heart, near its apex; a rough border on the left ſide of the diaphragm, as if the lungs had been glued to that part and torn off again; a partial adheſion of the lungs to the pleura; and a little purulent fluid within the pericardium. Certainly theſe were ſome ſigns of a ſlight inflammation having attacked the membranes inveſting the contents of the thorax. Neither can we ſuppoſe ſuch appearances to have exiſted without occaſioning ſome uneaſineſs: they were, perhaps, ſufficient to account for

that

that great tendernefs and oppreffive pain which the doctor felt from the leaft preffure on the fternum, or upon any part of the breaft near it.

The principal feat of the difeafe which proved fo tedious, and in the end fo fatal, was, no doubt, confined to the colon only; and it was entirely within the gut. The part firft affected muft have been that portion of the canal in which we obferved the moft mifchief. The fuperficial extent of the difeafe over fo large a furface as the whole arch of the colon, and the more formidable appearance of it, in only a few inches of the fame gut, diftinguifhed the part where the difeafe firft began, and where it muft have had its longeft duration.

The caufe of all this mifchief was conjectural with Dr. MATY himfelf. Had it arifen, as he fufpected, from having bruifed his fide with the hilt of his fword, we then fhould have found the gut injured from without inwards. But is it not moft likely, that a little bit of bone, the ftone of fruit, fome fharp or hard body, in paffing, had injured the gut fo much, as to lay a foundation for all the growing complaints? Nearly the fame appearances have been obferved in the oefophagus from only a hard cruft of bread lodging for a time in the paffage; which, after being forced down, was fucceeded by great forenefs, inflammation, ulceration, and at length

fo

ſo complete an obſtruction, as to occaſion the death of the patient; of which I once ſaw a very deplorable inſtance.

The ulcerated inteſtine is a diſeaſe generally, as in the caſe before us, ſlow in its progreſs, but certainly fatal. An accumulation of acrid matter, confined air, ſolid in-geſta, in ſhort any thing capable of ſtretching, irritating, or hardening the gut, will ſpread and increaſe the diſeaſe.

The faſciculated appearance in the rectum is what I have once met with in a very ſound gut, where the vil-lous coat was not in the leaſt injured; it is therefore ſometimes an original conformation, but apparently un-neceſſary, as the gut, we may preſume, would perform its office much more agreeably without it.

H. WATSON.

XXXII. *An Account of some Experiments made with an Air-pump on Mr.* Smeaton's *Principle; together with some Experiments with a common Air-pump. By Mr.* Edward Nairne, *F. R. S.*

AS the following experiments were made principally to try the performance of Mr. SMEATON's pear-gage, it may be proper to describe it, which I shall do in his own words, taken from the Phil. Transf. for the years 1751 and 1752, vol. XLVII. p. 420.

" I have found," says Mr. SMEATON, " the gages that
" have been hitherto made use of, for measuring the
" expansion of the air, very unfit to determine in an ex-
" periment of so much nicety; I have therefore con-
" trived one of a different sort, which measures the ex-
" pansion with certainty to much less than the 1000th
" part of the whole. It consists of a bulb of glass, some-
" thing in the shape of a pear, and sufficient to hold
" about

" about half a pound of quickfilver: it is open at one
" end, and at the other is a tube, hermetically clofed
" at the top. By the help of a nice pair of fcales I found
" what proportion of weight a column of quickfilver, of
" a certain length, contained in the tube, bore to that
" which filled the whole veffel: by thefe means I was
" enabled to mark divifions upon the tube anfwering to
" a 100oth part of the whole capacity; which being
" about one-tenth of an inch each, may, by eftimation,
" be eafily fub-divided into fmaller parts. This gage,
" during the exhaufting of the receiver, is fufpended
" therein by a flip wire. When the pump is worked as
" much as fhall be thought neceffary, the gage is pufhed
" down till the open end is immerged in a ciftern of
" quickfilver placed underneath; the air being then let
" in, the quickfilver will be driven into the gage till the
" air remaining in it becomes of the fame denfity with
" the external, and as the air always takes the higheft
" place, the tube being uppermoft, the expanfion will be
" determined by the number of divifions occupied by
" the air at the top.

" The degree to which I have been able to rarify the
" air in an experiment, has generally been about 1000
" times, when the pump is put clean together; but the

3.

" moifture

" moifture that adheres to the infide of the barrel as
" well as other internal parts, upon letting in the air, is
" in the fame fucceeding trials worked together with the
" oil, which foon renders it fo clammy as to obftruct
" the actions of the pump upon a fluid fo fubtil as the
" air when fo much expanded; but in this cafe it feldom
" fails to act upon the air in the receiver till it is ex-
" panded 500 times." Thus far Mr. SMEATON's ac-
count.

The pump with which the chief of the following ex-
periments were made, had the leather of its pifton foaked
in oil and tallow (and oil in the barrel) and every pre-
caution was taken that no water fhould get into the
working parts of the pump, except what might arife in
vapour from the fubftances which were under the
receiver.

EXPERIMENT I.

Having provided a pear-gage agreeable to Mr. SMEA-
TON's defcription, on which the fpace of a 4000th part
of the whole capacity was two-tenths of an inch; this
gage, together with a glafs cup which ferved as a ciftern

to

to hold the quickſilver for it, was put under a receiver; which receiver was placed on a leather ſoaked in oil and tallow, on the plate of the pump.

I muſt obſerve here, that the foot of the glaſs cup, which held the quickſilver for the pear-gage, being broken off by an accident, another foot, made of a piece of box-wood, was cemented to it. The reaſon of mentioning this circumſtance, which may ſeem trifling, will appear in the ſixth experiment. The pump was then worked, and the pear-gage was puſhed down till its open end was immerſed in the quickſilver in the cup: the air being then let into the receiver, it forced the quickſilver into the gage till it was filled within a 4000th part of the whole, ſo that by this gage the pump appeared to have expanded the air 4000 times. To what it might be owing that this pump, which was on Mr. SMEATON's principle, ſhould expand the air 4000 times inſtead of only 1000 as Mr. SMEATON's, I could not even ſurmiſe.

Having the pleaſure of knowing Mr. SMEATON, and being well acquainted with his great abilities in practice as well as theory, I could not imagine that this apparent ſuperiority could proceed from our having executed the various parts of the pump in a more perfect manner than he

he had done. I therefore determined, for greater cer-
tainty, to fee if the fault might not be in the gages; and
for that purpofe I repeated the experiment with the
fyphon-gage, and both the long and fhort barometer-
gages, and found that the feveral degrees of exhauftion
indicated by thefe, were very different from that which
the pear-gage had indicated: no conclufion, therefore,
could be drawn from this experiment.

I determined next to compare again this pear-gage
with the long and fhort barometer-gages with all the ac-
curacy I was mafter of: and firft, left the tubes of thefe
barometer-gages might not be perfectly clean and free
from moifture, I had fome tubes made at the glafs houfe;
and as foon as they were brought home, which was
within an hour after they were made, two of them, which
were of the fame fize, were filled with diftilled quickfil-
ver; and then the quickfilver was carefully boiled in the
tubes the whole length, which was about thirty-fix inches:
I then cut off about fix inches from the fealed end of one
of the tubes, and took care to keep it perfectly full of the
boiled quickfilver; it was then inverted into a glafs ciftern
containing boiled quickfilver; and a piece of very thin
ivory, about half an inch in length, with divifions on its
edge, was put over the tube, fo as to float on the furface
                                                        of

of the quickfilver in the ciftern, by which means the difference of the two furfaces could be feen to a great nicety. This kind of gage is called the fhort barometer-gage.

The other tube, which was cut off to thirty-three inches, being perfectly full of the boiled quickfilver, was alfo carefully inverted into a glafs ciftern containing boiled quickfilver, to fuch a depth that from the furface of the quickfilver in the ciftern to the top of the tube was twenty-nine inches; this had likewife a piece of ivory, with divifions on it, put over the tube, fo as to float on the furface of the quickfilver in the ciftern, in the fame manner as the other.

This long tube and the fhort barometer-gage being put both of them at the fame time under the receiver, which was carefully cemented to the plate of the pump; the pump was then worked for ten minutes, and the furface of the quickfilver in both the tubes came down very nearly to within one-twentieth or five hundredth parts of an inch of the furface of the quickfilver in their refpective cifterns.

The air was then let in, and the receiver being taken from the pump, the long tube was raifed up fo far in the ciftern as to let the quickfilver come down from the top of the tube, fo that it now became a common barometer,

and

and its height from the furface of the quickfilver in the ciftern meafured thirty inches; which agreed exactly with an open ciftern-barometer I had in the room. The quickfilver in the tube of this barometer was alfo boiled, and the meafure the fame.

The quickfilver was then emptied out of this long tube, and the fealed end being cut off, it was then cemented to a piece of brafs, by which means it was fcrewed to the air-pump; and the lower end being immerfed in a cup of boiled quickfilver, it then made that kind of gage where the air is taken from the top of the tube, and which is called the long barometer-gage. This gage being fixed to the pump, and the fhort barometer-gage put on the plate of the pump under a receiver, the receiver was cemented to the plate of the pump, and the pump worked for ten minutes as before. The quickfilver in the fhort barometer-gage fell now nearly to within one-twentieth or five hundredth parts of an inch of that in the ciftern, and the quickfilver in the long gage rofe nearly to within a twentieth or five hundredth parts of an inch of the height it was at when it was made as a common barometer.

Gages made with thefe precautions feem to me to be the moft to be depended upon, in determining the actual

diminution

diminution of the preffure on the furface of the quick-
filver in the tube of the long gage, and alfo on the fur-
face of the quickfilver in the ciftern of the fhort gage.
But of thefe two gages the long barometer-gage was
chiefly ufed in the following experiments, as being fixed
to the pump: however, having now made thefe two
gages with as much accuracy as I was mafter of, and
finding that they agreed pretty nearly, I proceeded to
repeat my firft experiment.

EXPERIMENT II.

I put the fhort barometer-gage, and the pear-gage
with the glafs cup having a wooden foot, both toge-
ther under the receiver, which receiver was placed on a
leather foaked in oil and tallow on the plate of the
pump; the pump was then worked for ten minutes, and
the quickfilver was brought down in the fhort barome-
ter-gage to about one-tenth of an inch of the furface of
the quickfilver in the ciftern, and rofe in the long baro-
meter-gage to within one-tenth of an inch of the height
of the quickfilver in my ftandard barometer, which was
at that time at thirty inches; by which it appeared, that

the

the preffure on the furface of the quickfilver in the ciftern, and in the tube of the long barometer-gage, was diminifhed to about a three-hundredth part: the pear-gage being now pufhed down till its open end was im-merfed under the furface of the quickfilver in the cup, the air was then let in, and the pump appeared by that gage to have exhaufted all but a fix thoufandth part of the air; or, in other words, the degree of exhauftion by this gage appeared to be fix thoufand times.

Finding ftill this difagreement between the pear-gage and the other gages, I tried a variety of experiments; but none of them appeared to me fatisfactory, till one day in April 1776, fhewing an experiment with one of thefe pumps to the honourable HENRY CAVENDISH, Mr. SMEATON, and feveral other gentlemen of the Royal Society, when the two gages differed fome thoufand times from one another, Mr. CAVENDISH accounted for it in the following manner. " It appeared," he faid, " from fome experiments of his father's, Lord CHARLES " CAVENDISH, that water, whenever the preffure of the " atmofphere on it is diminifhed to a certain degree, is " immediately turned into vapour, and is as immediately " turned back again into water on reftoring the pref- " fure. This degree of preffure is different according to " the heat of the water: when the heat is 72° of FAH-

" RENHEIT's

" RENHEIT's fcale, it turns into vapour as foon as the
" preffure is no greater than that of three quarters of an
" inch of quickfilver, or about one-fortieth of the ufual
" preffure of the atmofphere; but when the heat is only
" 41°, the preffure muft be reduced to that of a quarter
" of an inch of quickfilver before the water turns into
" vapour. It is true, that water expofed to the open air
" will evaporate at any heat, and with any preffure of
" the atmofphere; but that evaporation is intirely owing
" to the action of the air upon it: whereas the evapora-
" tion here fpoken of is performed without any affiftance
" from the air. Hence it follows, that when the receiver
" is exhaufted to the above-mentioned degree, the moif-
" ture adhering to the different parts of the machine
" will turn into vapour and fupply the place of the air,
" which is continually drawn away by the working of
" the pump, fo that the fluid in the pear-gage, as well as
" that in the receiver, will confift in good meafure of va-
" pour. Now letting the air into the receiver, all the
" vapour within the pear-gage will be reduced to water,
" and only the real air will remain uncondenfed; confe-
" quently the pear-gage fhews only how much real air
" is left in the receiver, and not how much the preffure
" or fpring of the included fluid is diminifhed, whereas
" the

" the common gages fhew how much the preffure of
" the included fluid is diminifhed, and that equally,
" whether it confift of air or of vapour."

Mr. CAVENDISH having explained fo fatisfactorily the
caufe of the difagreement between the two gages, I con-
fidered, that, if I were to avoid moifture as much as poffi-
ble, the two gages fhould nearly agree: this induced
me to make the following experiment.

EXPERIMENT III.

The plate of the pump being made as clean and as
dry as poffible, there was then put on it the before-
mentioned fhort barometer-gage, alfo the pear-gage
with a ciftern entirely of glafs which held the quick-
filver; they were then covered with a receiver, round
the outfide of which was laid a cement which per-
fectly excluded the outward air; every part, before it
was put under the receiver, as well as the receiver itfelf,
being made as clean and as free from moifture as poffi-
ble[a].   The pump was then worked for ten minutes,

(a) It may be proper here to take notice, that the pump in every experiment
hereafter mentioned was worked ten minutes, and the fame receiver continued
cemented to the pump-plate, except where it is otherwife mentioned. The top
part of this receiver was made to open, in order to put in different things.

and the barometer-gages indicated a degree of exhauftion nearly 600; the air was then let into the receiver, the pear-gage indicated a degree of exhauftion, but very little more than 600 alfo. The near agreement of the pear-gage with the barometer-gages in this laft experiment, in which I had been fo careful to exclude the moifture as much as poffible, feemed to prove beyond a doubt, that their difagreeing in the firft and fecond experiments muft have been owing (as Mr. CAVENDISH fuppofed) to the moifture which in them had not been fo carefully excluded. But I began now to fufpect alfo, that there might arife a vapour from fome moifture that might be contained in the leather foaked in oil and tallow, or in the wooden foot which was cemented to the glafs cup, both ufed in the firft and fecond experiments: thefe fufpicions induced me to try the following experiments.

EXPERIMENT IV.

A piece of leather dreffed in allum, known by the name of white fheep-fkin, of about four inches diameter, which had been foaked in oil and tallow about a year ago (fuch as was ufed to place the receiver on in the firft and fecond experiments) was put into the receiver; the

pump was then worked, and the barometer-gage indi-
cated a degree of exhauftion of nearly 300; but on the
admiffion of the air the pear-gage indicated a degree of
exhauftion of 4000.

EXPERIMENT V.

The piece of leather being taken out, the pump was
then worked, and the degree of exhauftion appeared by
both the barometer and pear-gages to be about 600, as
in the third experiment.

EXPERIMENT VI.

A cylinder made of a piece of box wood (which I had
kept by me for more than a year) one inch in diameter
and three inches in length, was put into the receiver
(this piece of wood was of the fame kind as that which
was cemented to the foot of the glafs cup ufed in the firft
and fecond experiments) the pump was then worked,
and the degree of exhauftion appeared by the barometer-
gage to be 300, but by the pear-gage 16,000.

Thefe experiments have often been repeated, but the
refult was feldom the fame. When leather foaked in oil
and tallow has been put into the receiver, the pear-gage
has fometimes indicated a degree of exhauftion of
20,000,

20,000, and fometimes no more than 500; it likewife differs very much with the box wood, which may perhaps be owing to different degrees of heat and moifture.

From thefe experiments it is evident, that there arifes an elaftic vapour from the leather dreffed in allum and foaked in oil and tallow, and alfo from the piece of box wood, when the weight of the atmofphere has been partly taken off by the action of the pump; and that this vapour preffes upon the furface of the quickfilver in the tube of the long barometer-gage, and of that in the ciftern of the fhort one; and that, confequently, the teftimony of both thefe gages muft be influenced by this vapour, as well as by the fmall remainder of common air: but as it is the nature of the pear-gage not to give its teftimony till the remaining air contained in it is preffed, fo as to become of the fame denfity of the atmofphere; and as this vapour cannot fubfift in the form of vapour under that preffure, this gage is not at all influenced by it, but indicates the remaining quantity of permanent air only.

Seeing thus what a confiderable quantity of vapour arofe from the compound of leather, allum, oil, and tallow, my next object was to find out from which of thofe fubftances it chiefly arofe; how far I have fucceeded will appear by the following experiments.

Sub-

Subftances put into the receiver.	Weight when put into the receiver.	Degree of exhauftion according to	
		Barom. gage.	Pear-gage.
EXP. VII. Tallow, — —	2 ounces	431	600
EXP. VIII. Oil, — —	2 ounces	377	480
EXP. IX. Allum, — —	2 ounces	378	580
EXP. X. A piece of leather as it came from the leather-fellers, — — —	100 grains	152	100,000.
EXP. XI. The fame piece of leather foaked in the fame two ounces of tallow and oil melted together, — —		432	800

From thefe experiments it appears, that the elaftic vapour which caufed fo great a difference in the teftimony of the gages, arofe principally from the leather, and but little from the tallow, oil, or allum: it even appears by the tenth experiment, that it came from the leather, and fupplied the place of the exhaufted air fo faft, that I could not (at leaft in the ten minutes) make the barometer-gage indicate a degree of exhauftion of of more than 159.

To determine whether it was the moifture in the leather from which the vapour arofe, I made the following experiments.

Sub-

Substances put into the receiver.	Weight when put into the receiver.	Degrees of exhaustion according to		Variation in weight during the experiments.
		Barom. gage.	Pear-gage.	
EXP. XII. A piece of white leather, fresh from the leather-fellers, — —	100 grains	134	100,000	lost 2 grains.
EXP. XIII. The same piece of leather, dried by the fire till it would lose no more of its weight, — —	80 grains	268	280	gained 2 grs.
EXP. XIV. The same piece of leather held in the steam of hot water till it had regained the 20 grains it had been deprived of, — —	100 grains	147	100,000	lost 2 grains.

In this last experiment it was full three quarters of an hour before the leather regained the twenty grains of weight, although it was held very near the surface of the hot water.

Whenever I have asserted, that the degree of exhaustion, according to the pear-gage, was so great as 100,000, I only guessed it to be thereabouts, for my gage is not graduated to more than 4000; but, that it may be seen what reason I had to suppose it at 100,000, I have brought my pear-gage filled in this last experiment, for the inspection of the gentlemen present.

EXPERIMENT XV.

The same piece of leather used in the eleventh experiment was put into a damp cellar, where it was left till

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the next day; it was then put again into the receiver, and the degree of exhauftion, according to the barometer-gage, was 300, and according to the pear-gage 3500.

Being now perfectly fatisfied that the variation in the teftimony of the pear and barometer-gages was occafioned by the moifture contained in the fubftances I had put into the receiver affuming the form of vapour; I determined next to try what would be the effect of the vapour which might arife from fmall quantities of different fluids, and from fome other fubftances containing moifture of various kinds.

Subftances put into the receiver.	Weight when put in.	Degree of exhaufting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XVI. Water in a watch-glafs,	3 grains	148	24,000	loft 1½ grain.
EXP. XVII. Water in a glafs cup, diameter two inches, —	100 grains	89	8000	loft 2 grains.
EXP. XVIII. Spirit of wine in the fame cup, —	100 grains	54	6000	loft 9 grains.
EXP. XIX. Vitriolic acid, —	100 grains	340	220	gained 1 gr.
EXP. XX. A piece of the infide of a china orange with fome of the rind, —	100 grains	160	100,000	loft 2¼ grs.
EXP. XXI. A piece of the infide of an onion, —	100 grains	160	100,000	loft 1¼ grain.
EXP. XXII. A piece of tainted beef,	100 grains	152	100,000	loft 2¼ grs.
EXP. XXIII. A piece of frefh beef,	100 grains	136	100,000	loft 2½ grs.
EXP. XXIV. Spirit of turpentine,	100 grains	301	1800	loft 2 grains.
EXP. XXV. Pearl-afh, —	2 ounces.	118	5000	
EXP. XXVI. The fame pearl-afh made very hot, —		198	420	
EXP. XXVII. A lighted candle held in the receiver till it went out, — —		297	1800	

Subſtances put into the receiver.	Weight when put in.	Degrees of exhauſting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XXVIII. A piece of charcoal,		129	1800	
EXP. XXIX. The receiver heated by holding ſeveral pieces of lighted charcoal in it, and then the above piece being thoroughly lighted was put into the receiver, and the pump worked, —		650	600	
EXP. XXX. Camphire, —	100 grains	304	520	{ loſt barely ½ a grain.
EXP. XXXI. Sulphur made to burn on a piece of braſs, —		247	320	

Obſerving by theſe experiments that the ſmall quantity of moiſture which exhaled from the ſubſtances under the receiver prevented the pump from exhauſting it to any very conſiderable degree, I began to ſuſpect that whenever wet leather had been uſed to connect the receiver with the plate, there muſt have riſen ſo great a quantity of vapour as to have prevented the degree of exhauſtion from being near ſo great as in ſome of the foregoing inſtances. Theſe ſuſpicions induced me to make the following experiments.

	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
EXP. XXXII. The receiver was taken off, and after the cement was wiped clean from it, and every part made perfectly dry, it was put again on the pump-plate, and a little oil only was poured round the outside edge,	nearly 600	full 600
EXP. XXXIII. The receiver was taken off again, and instead of the oil it was set on a piece of leather, which had been soaked two days in water, —	51	16,000
EXP. XXXIV. The last experiment repeated with the same piece of leather, — —	51	1500
EXP. XXXV. The last experiment repeated again with the same piece of leather, — —	51	1000
EXP. XXXVI. The receiver was taken off, and instead of the leather soaked in water, there was put on a piece of the same sort of leather, soaked in a mixture of water and spirit of wine, such as Mr. SMEATON used,	47	12,000
EXP. XXXVII. The last experiment repeated with the same leather, — — —	47	1150
EXP. XXXVIII. The last experiment repeated again with the same leather, — —	47	500

The great difference in the testimony of the pear-gage in these six last experiments appeared to me exceedingly astonishing, for the leathers seemed each of them to be as moist at last as at first.

By these experiments I was convinced how effectually the use of leather soaked in water, or in water and spirit of wine, prevents the pump from exhausting to any considerable degree. I have made a number of experiments of the same kind as these; but have never been able to exhaust, under such circumstances, to a greater degree than between 50 and 60, when the heat of the room was

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about 57° by a thermometer of FAHRENHEIT's fcale: but the following experiments will fhew how much fome different degrees of heat affect the degree of exhauftion,

	Height of the Therm.	Degrees of exhauf-tion according to	
		Barom. gage.	Pear-gage.
EXP. XXXIX. Receiver fet on leather which had lain all night in water, —	46	84	20,000
EXP. XL. Receiver fet on a leather foaked all night in two parts water and one of fpirit of wine, — — —	46	76	8000

The pump having been put in a room of the heat of 57° of FAHRENHEIT's fcale for feven hours together, with the leathers put in the fame water and the fame fpirit of wine and water which they had been foaked in all night, and which had been ufed in the two laft experiments, the following experiments were made.

	Height of the Therm.	Degree of exhauf-tion according to	
		Barom. gage.	Pear-gage.
EXP. XLI. The receiver fet on the leather foaked in water, — —	57	56	16,000
EXP. XLII. Receiver placed on a leather foaked in water and fpirit of wine, —	57	49	1200

The following table will fhew the comparative excellency between the pump on Mr. SMEATON's principle with which the chief of thefe experiments have been tried,

tried, and one of my common double barreled table air-pumps under the fame circumftances. The leather on the piftons of both was foaked in oil and tallow, and the receiver cemented down to each plate; the pumps were both of them frefh oiled.

	Pump on Mr. SMEA-TON's principle. Degrees of exhauftion according to		Common pump. Degrees of exhauftion according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. XLIII. A piece of leather, weighing 100 grains, as it came from the leather-fellers, was put into the receiver of each pump, both pieces being cut from the fame fkin clofe by each other, —	152	100,000	108	12,000
EXP. XLIV. The fame pieces of leather dried by the fire till they would lofe no more of their weight, — —	506	520	160	165

The following experiments will fhew the effect of water ufed in the barrels of pumps to make the piftons move air tight in them.

I took the fame common air-pump ufed in the laft experiment, and having taken off the leathers foaked in oil and tallow from the piftons of this pump, and wiped the barrels as clean as poffible, I then put new leathers, which had been foaked in water, and new bladder valves; the receiver was then cemented to the pump-plate as before.

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	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
EXP. XLV. The pump was then worked as ufual, —	37	38
EXP. XLVI. The laſt experiment repeated with another common pump, the leathers of the piſtons of which were alſo ſoaked in water, — —	34	37

From theſe experiments it evidently appears, that the air-pump of OTTO GUERICKE, and thoſe contrived by Mr. GRATORIX, and Dr. HOOKE, and the improved one by Mr. PAPPIN, both uſed by Mr. BOYLE, alſo HAUKS-BEE's, S'GRAVESANDE's, MUCHENBROOK's, and thoſe of all who have uſed water in the barrels of their pumps, could never have exhauſted to more than between 40 and 50, if the heat of the place was about 57; and although Mr. SMEATON, with his pump, where no water was in the barrel, but where leather ſoaked in a mixture of water and ſpirit of wine was uſed to ſet the receiver on the pump-plate, may have exhauſted all but a thouſandth or even a ten thouſandth part of the common air, according to the teſtimony of his pear-gage; yet ſo much vapour muſt have ariſen from the wet leather, that the contents of the receiver could never be leſs than a ſeventieth or eightieth part of the denſity of the atmoſphere: never-theleſs, it does not ſeem that any deficiency in the con-ſtruction of Mr. SMEATON's pump was the cauſe of his

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not being able to exhauft beyond the low degrees of 70 or 80. Had he been aware of the bad effects of fetting the receiver upon leather foaked in water and fpirit of wine; and had he made ufe of the precaution to free all parts of his pump as much as poffible from moifture, I make not the leaft doubt but the air-pump, which he executed himfelf, would have exhaufted to as great a degree, as that pump has been feen to have done with which the chief of thefe experiments were made.

Having read the principal part of this paper to Mr. SMEATON, and fhewn him fome of the experiments; one in particular, where the pear-gage, as he obferved himfelf, was filled to no lefs than 100,000th part of the whole content; he remarked from memory, that he had in feveral trials exceeded 1000 times, and once, as he remembered, near or about 10,000 times; but as he never could account how this happened, which appeared to him perfectly accidental, and therefore could not depend upon doing it at pleafure, he contented himfelf with putting down 1000 times, as being what (under the circumftances mentioned in his papers) he had a tolerable certainty of.

I muft here again obferve, that if we only wifh to know the quantity of permanent air remaining in the receiver after it is as much exhaufted as poffi-

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ble, it feems, that it is by Mr. SMEATON's gage only
that we can know it. Again, when by the affiftance of
his gage and the barometer-gage together, we have dif-
covered that there is a vapour which arifes and occupies
the place of the permanent air which is exhaufted, it
feems that it is by the means of his gage only that we
can difcover what part of the remaining contents of the
receiver confifts of this vapour, and what part of perma-
nent air.

An account of ſome further experiments made with the
ſame air-pump on Mr. SMEATON's *principle, the reſults*
of which were different from the former.

AFTER I had made the foregoing experiments, and
thought to have done with the fubject (for fome time at
leaft) in reviewing them for the laft time, I perceived one
or two, the extraordinary refults of which (though not
unnoticed by me before) I now thought I had not paid
fufficient attention to. Experiment the 19th (in which
I found that when vitriolic acid was put into the receiver,
and the pump worked for the ufual time, the pear-
gage indicated a much lefs degree of exhauftion than the

barometer-gage) feemed to me now fo furprizing, that it
was impoffible not to wifh to repeat this experiment with
all the care poffible, and to endeavour to recollect all the
circumftances which I thought could any way influence
the refult.

The vitriolic acid I made ufe of in the following ex-
periments was fome that I had had by me for fome time;
it had been kept in a phial, ftopped with a glafs-ftopper,
and tied over with a bladder. The thermometer in the
room was at 59°, and the weather remarkably dry.

	Weight when put into the receiver.	Degrees of exhauf-tion according to		Variation of weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XLVII. Vitriolic acid in a glafs cup, two inches diameter,	100 grains	602	380	gained 1 gr.
EXP. XLVIII. The laft experi-ment repeated with the fame vitriolic acid in the fame cup,	101 grains	502	350	gained ½ a gr.
EXP. XLIX. The former experi-ment repeated again with the fame vitriolic acid in the fame cup,	101½ grs.	502	350	
EXP. L. The former experiment repeated the fourth time in every refpect as before,	101½ grs.	502	340 +	{ gained a ¼ of a grain.

The generation of vapour in the exhaufted receiver
which Mr. CAVENDISH had fuppofed, and which I
feemed, by my former experiments, to have proved, ap-
peared fatisfactorily to have accounted for the pear-

gage's indicating a greater degree of exhauftion than the barometer-gage: but what to fuppofe could poffibly make it indicate a lefs, I was entirely at a lofs; for after having made thefe furprizing experiments on the vitriolic acid, I wifhed once more to try if I could repeat, with the fame refult, fome of my former experiments, in which the pear-gage had indicated fo nearly the fame degree of exhauftion as the barometer-gages. Accordingly I took away the vitriolic acid from under the receiver; the pump was then frefh oiled, and I was very careful to wipe clean and dry the receiver and pump-plate, and then cemented down the receiver as ufual.

EXPERIMENT LI.

The pump was then worked, and the degree of exhauftion appeared by the barometer-gage to be 432; but by the pear-gage to be but 370.

I know of no circumftance attending this experiment that differed from thofe in which my former experiments were made when the gages agreed fo nearly, unlefs it were that of the weather: I recollect that it was then very damp, and now it had been very dry for fome time. How this circumftance could make fo great an alteration in the refult of thefe experiments, I cannot pre-

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tend to fay; but fome of the following experiments will fhew that the pear-gage ftill continued in many cafes to indicate a lefs degree of exhauftion than the barometer-gage.

In all the preceding experiments the pump was worked for ten minutes, and the pear-gage was at the end of that time pufhed down fo as for the mouth to be immerfed in the ciftern of quickfilver, and the air then let in according to the manner of ufing this gage; but now that I found that the teftimony of this gage fo feldom agreed with that of the barometer-gage, I wifhed to try, whether they might not agree when the receiver was exhaufted only in part, though they did not when it was exhaufted as much as poffible. For this purpofe I had the fame receiver fitted with two pear-gages, fo that I now could immerfe the mouth of one of them in the ciftern of quickfilver when I had exhaufted the receiver in part only, and not immerge the mouth of the other till the receiver was exhaufted for the ufual time of ten minutes; in which time I found I could always raife the quickfilver in the barometer-gage as high as if I were to work the pump much longer.

EXP.

	Degrees of exhauſtion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. LII. The receiver cemented to the pump-plate, —	430	300	430	360
EXP. LIII. The receiver unce-mented, wiped clean from the cement, and put on the pump-plate, with a little oil round the outſide, —	502	360	502	360
EXP. LIV. The receiver put on a leather ſoaked in oil and tallow,	502	320	323	500

This laſt experiment ſeemed very extraordinary; for after having worked the pump for five minutes only, the barometer-gage indicated a degree of exhauſtion of 502; but by working the pump five minutes more, it indicated a leſs degree of exhauſtion, *viz.* 323. This effect I have obſerved to happen more than once.

	Degrees of exhauſtion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. LV. Receiver put on a leather ſoaked in water for one night,	47	380	63	8000
EXP. LVI. Receiver put on a leather ſoaked for one night in a mixture of two parts water and one ſpirit of wine, — —	48	300	50	1200

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The receiver was then taken off from the pump-plate, and a blank fcrew fcrewed into the hole in the pump-plate.

EXPERIMENT LVII.

The pump was then worked for ten minutes without any receiver on the pump-plate. The barometer-gage alone being afterwards connected with it, the gage indicated a degree of exhauftion of 50 only, which was the fame as in the laft experiment; fo that the bad effects of the mixture of the fpirit of wine and water ftill continued.

I then poured about two fpoonfuls of oil down the hole in the pump-plate, and the piftons were worked gently till moft of the oil had paffed through the pump into a refervoir made to receive it. A blank fcrew was then fcrewed into the hole in the pump-plate as before, and after the pump had been worked for a minute or two, four or five times, the air being let in between each time, the oil had then wafhed fo much of the moifture out of the infide of the pump, that I was now able to exhauft to 430 inftead of only 50 times, as before the oil had been made to pafs through it.

During the courfe of thefe experiments on the air-pump it appeared, by the teftimony of the pear and baro-

meter-gages, that the remaining contents of a receiver, when exhaufted as much as poffible, was at different times of different kinds; fometimes it feemed to confift entirely of permanent air, as when a little vitriolic acid, &c. was put in the receiver; and fometimes moftly of vapour arifing from moifture, and but a very fmall proportion of permanent air, as when a bit of damp leather, &c. was in the receiver. I was now therefore defirous of feeing what appearance the electric matter would exhibit in thefe different rarified media.

For this purpofe I had a glafs tube made, of an inch bore, and four feet and a half in length. This tube was connected to the receiver of the air-pump by means of an elbow-piece of brafs, to which it was cemented; which elbow-piece was inferted perpendicularly in the top of the receiver: as the elbow made a right angle, the tube itfelf was of courfe horizontal.

Moreover, at that end of the glafs tube which was cemented to the brafs elbow-piece, there was fixed, on the infide, a piece of brafs wire, about three inches and a half long, filed to a point, and pointing towards the other end of the tube. At the other end of the tube was cemented a brafs fcrew fitted to a brafs cap, which fcrewed on it; and in this brafs cap was fixed a brafs wire, three inches and a half long, which pointed towards

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the brafs point; at the other end of the tube, and at the end of this wire, was a brafs ball, eight-tenths of an inch diameter.

The brafs cap at this extremity of the tube which is fartheft from the receiver was made round, and placed fo as to be in contact with the prime conductor of an electrical machine.

I now firft put fome vitriolic acid into the receiver, as a means of being able to make the remaining contents of the receiver, when exhaufted as much as poffible, to confift of permanent air only, unadulterated with vapour; and as the receiver was the fame I had ufed in my foregoing experiments, there were two pear-gages fitted to it, which pear-gages I pufhed down into the cifterns of quickfilver at different times, and the pump was worked as in my former experiments for ten minutes: heat of the room 59°.

The electrical machine was worked during the whole of the experiments.

EXPERIMENT LVIII.

Electrical appearances exhibited.	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
Light began first to appear in flashes, — —	5	
Light appeared the whole length of the tube in striæ,	8	
Tube was filled with an uniform body of pale light,	74	.75
The pump had now been worked five minutes.		
The pump was then worked five minutes more.		
The tube was still filled with a uniform body of pale light, — — — —	269	230

The conductor being then removed to a distance from the tube, it was made to approach it by degrees till a spark struck it, which was at the distance of two inches; the light in the tube now appeared like a compact body of fire, of a vivid purple colour, tending to a red.

Objects were seen through the tube when filled with this body of the electrical light, no less distinctly than if there had been no such light in the tube [a].

The vitriolic acid being taken out of the receiver, I put a piece of leather of 100 grains, as it came from the leather-sellers, into that end of the tube which was next to the conductor of the electrical machine, and farthest from the receiver of the air-pump. I put the leather in

(a) This circumstance has been before remarked by Dr. HAMILTON in his conjectures on the tails of comets.

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at this end of the tube rather than into the receiver, to be fure that the tube might be filled with the vapour arifing from the damp leather rather than with the common air. The pump and electrical machine were then worked as before.

EXPERIMENT LIX.

Electrical appearances.	Degrees of exhauftion according to	
	Barom. gage.	Pear-gage.
Light began firft to appear in flafhes, — —	12	
Light appeared the whole length of the tube in ftriæ,	22	
Light vanifhed fcarce to be feen, — — —	90	100
The pump had now been worked feven minutes. The conductor was now removed from the tube, and the greateft ftriking diftance was found to be one inch,		
The tube now appeared luminous, but the light was faint and white. The conductor was then again put in contact with the tube, and the machine worked: the pump was alfo worked for three minutes more, but fcarce any light appeared, — —	148	20,000

The conductor was then again removed from the tube, and the ftriking diftance was found to be only one inch and four-tenths: the tube at the time of ftriking was luminous as before, and the light was of the fame faint white colour.

Having lately received from my friend Dr. LIND fome æther prepared by the ingenious Mr. WOLFE, I was very defirous to try whether I could produce any confiderable

degree

degree of cold by the evaporation of æther under a re-
ceiver whilft exhaufting. For this purpofe I put the
æther into a phial, the neck of which was fufficient to
admit the ball of a thermometer: this being placed on
the air-pump, under a receiver which had a plate at the
top, with a wire paffing through a collar of leathers; to
this wire the thermometer was fixed, by which means
I could eafily dip the ball of the thermometer into the
æther.

EXPERIMENT LX.

The pump was now worked, and whilft the receiver
was exhaufting, the ball of the thermometer was often
dipped into the æther; and when the degree of exhauf-
tion by the barometer-gage was 65 (which was the ut-
moft in this cafe that the pump would exhauft to) the
degree of cold indicated by the fall of the quickfilver in
the thermometer was 48° below o on FAHRENHEIT's
fcale; fo that there was a degree of cold produced 103°
colder than the air in the room where the experiment
was made, the thermometer in it being at 55° degrees
above o. The pump was kept continually working for
half an hour, and the ball of the thermometer often
dipped into the æther; but no greater degree of exhauf-
tion or cold could be produced. The air being let into
the

the receiver, the quickfilver in the thermometer rofe
10°, *viz.* to 38° below o.

EXPERIMENT LXI.

Frefh æther being put into the phial to what was re-
maining, the thermometer rofe to 30° above o : the pump
was then worked again conftantly for half an hour; yet
by the barometer-gage the degree of exhauftion was
now not more than 16, and the degree of cold produced
did not exceed the 11th degree below o, as appeared by
the quickfilver in the thermometer. The air being let
into the receiver, the remaining æther was examined,
and there were found feveral pieces of ice at the bottom
of the phial, fome of them as big as large peas, which,
when the æther became nearly of the heat of 32° or
freezing point of water, were intirely diffolved.

The air-pump with which thefe experiments were
made exhaufted above 400 times before the æther was
put under the receiver.

XXXIII. *On the Culture of Pine-apples. An extract of a Letter from* William Baftard, *Efq. of* Kitley *in* Devon-fhire, *to* Samuel Mufgrave, *M. D. F. R. S. dated* Kitley, March 15, 1777. *Communicated to the Society by Dr.* Mufgrave.

Read June 19, 1777.

BEFORE I enter into the particulars of raifing pine-apples in water, it will be neceffary to tell you that my hot-houfe is covered with the beft crown-glafs, which I apprehend gives more heat than the common fort of green glafs generally ufed for hot-houfes. In the front part of the houfe, and indeed any where in the loweft parts of it, the pine-apple plants will not thrive well in water. The way in which I treat them is as follows. I place a fhelf near the higheft part of the back wall, fo that the pine-plants may ftand without abfolutely touching the glafs, but as near it as can be: on this fhelf I place pans full of water, about feven or eight inches deep; and in thefe pans I put the pine-apple plants, growing in the fame pots of earth as they are generally planted in to be plunged into the

bark-

bark-bed in the common way; that is, I put the pot of earth, with the pine-plant in it, in the pan full of water, and as the water decreafes I conftantly fill up the pan. I place either plants in fruit, or young plants as foon as they are well rooted, in thefe pans of water, and find they thrive equally well: the fruit reared this way is always much larger, as well as better flavoured, than when ripened in the bark-bed. I have more than once put only the plants themfelves without any earth, I mean after they had roots, into thefe pans of water, with only water fuf-ficient to keep the roots always covered, and found them flourifh beyond expectation. In my houfe, the fhelf I mention is fupported by irons from the top, and there is an intervening fpace of about ten inches between the back wall and the fhelf. A neighbour of mine has placed a leaden ciftern upon the top of the back flue (in which, as it is in contact with the flue, the water is always warm when there is fire in the houfe) and finds his fruit excellent and large. My fhelf does not touch the back flue, but is about a foot above it; and confe-quently the water is only warmed by the air in the houfe. Both thefe methods do well. The way I account for this fuccefs is, that the warm air always afcending to the part where this fhelf is placed, as being the higheft

part

part of the houſe, keeps it much hotter than in any other part. The temperature at that place is, I believe, ſeldom leſs than what is indicated by the 73d degree of FAHRENHEIT's thermometer, and when the Sun ſhines it is often at above 100°: the water the plants grow in ſeems to enable them to bear the greateſt heat, if ſufficient air be allowed; and I often ſee the roots of the plants grow- ing out of the holes in the bottom of the pot of earth, and ſhooting vigorouſly in the water.

My hot-houſe (the dimenſions of which it may be proper to know) is ſixty feet long, and eleven feet wide the flues included; ſix feet high in the front, and ele- ven feet at the back on the inſide of the houſe. It is warmed by two fires. A leaden trough or ciſtern on the top of the back flue is preferable to my ſhelf, as in it the pine-plants grow much faſter in the winter, the water being always warmed by the flue: of this I have ſeen the great benefit theſe laſt two months in my neighbour- hood. It is not foreign to this purpoſe to mention that, as a perſon was moving a large pine-plant from the hot- bed in my houſe laſt ſummer, which plant was juſt ſhowing fruit, by ſome accident he broke off the plant juſt above the earth in which it grew, and there was no root whatever left to it: by way of experiment I

took

took the plant, and fixed it upright in a pan of water (without any earth whatever) on the ſhelf; it there ſoon threw out roots, and bore a pine-apple that weighed upwards of two·pounds.·

XXXIV. *Experiments and Observations made in* Britain, *in order to obtain a Rule for measuring Heights with the Barometer.* By Colonel William Roy, *F. R. S.*

INTRODUCTION.

IN philofophical inquiries of every kind, where any point is to be afcertained by experiments, thefe cannot be repeated too often, nor varied too much, in order to obtain the truth: for even when the utmoft precaution hath been ufed, and the greateft pains have been taken, it rarely happens, that they agree fo exactly, as to leave no room for doubt. Were it poffible at all times, to have experiments made in circumftances perfectly fimilar, a confiderable degree of confiftency might naturally be expected among the refults, whereof the mean would determine the point in queftion; but different men, making ufe of different inftruments, have different modes of conducting their operations, each purfuing the tract that feems to him the moft likely to infure fuccefs. Hence it is that a variety

of

of refults arife, and that things fometimes appear contra-
dictory, or at leaft prefent themfelves under new forms,
difficult at firft fight to be accounted for, and therefore
apt to miflead, till by a farther inveftigation of the mat-
ter, the true caufes are difcovered. Even irregularities of
this fort are worthy of being communicated, that others
may know what hath happened before, and what, in like
cafes, they may expect to meet with, in the courfe of
their future inquiries. Improvements of every kind ad-
vance by flow degrees; and it is not until things have
been viewed in every poffible light, that the errors,
even of our own experiments, are difcovered, the points
in queftion ultimately afcertained, and the branch of
philofophy depending upon them, gradually brought
nearer to perfection.

Ever fince the difcovery made by TORRICELLI, the
barometer hath been applied, by different perfons, in dif-
ferent countries, to the meafurement of vertical heights,
with more or lefs fuccefs, according to the more or lefs
perfect ftate of the inftruments ufed, and the particular
modes of calculation adopted, by the obfervers. But of
all thofe who have hitherto employed themfelves in this
way, none hath beftowed fo much time and pains, or
fucceeded fo well, as Mr. DE LUC, of Geneva, F. R. S. In
two quarto volumes, publifhed fome years fince, that

gentleman

gentleman hath given us the hiftory of the barometer
and thermometer, with a very curious and elaborate de-
tail of many years experiments, made by him, chiefly on
the mountain Saleve. It would be totally fuperfluous.
here to enter into any circumftantial account of the me-
thod he makes ufe of, fince that hath already been fo
fully illuftrated by two Fellows of the Royal Society,
who have at the fame time given formulæ and tables,
adapted to the meafures of this country, (Phil. Tranf. for
1774, vol. LXV. N° xx. and xxx.) that nothing farther
can be defired on that head.

It may neverthelefs be neceffary juft to call to remem-
brance that the rule, deduced from the obfervations on
Saleve, confifts of three parts. 1ft, The equation for the
expanfion of the quickfilver in the tube, from the effect
of heat, whereby the heights of the columns, in the in-
ferior and fuperior barometers, are conftantly reduced
to what they would have been in the fixed temperature
of $54°\frac{1}{4}$ of FAHRENHEIT, independant of the preffure
they refpectively fuftained. 2d, When the mean tem-
perature of the column of air to be meafured, is 69°.32,
as indicated by thermometers expofed to the Sun's rays
at its extremities; then the difference of the common
logarithms, of the equated heights of quickfilver in the

two

two barometers, gives the altitude intercepted between them, in toifes and thoufandth parts, reckoning the three figures to the right hand decimals, and the others integers, the index being neglected. This temperature of 69°.32, when the logarithmic differences give the real height without any equation, is reduced to 39°.74, the new zero of Mr. DE LUC's fcale, when his formula is adapted to Englifh fathoms and thoufandth parts, inftead of French toifes. And laftly, when the mean temperature of the air is above or below 39°.74, an equation, amounting to $\frac{21}{10000}$ parts of the logarithmic height for each degree of difference, is, in the firft cafe to be added to, and in the laft fubtracted from, that refult, in order to obtain the real altitude.

In Mr. DE LUC's book, the experiments for afcertaining the expanfion of the quickfilver, are not given in detail; neither are the particular temperatures of the barometers fpecified. The winter feafon was however chofen for the purpofe; one being left in a cold room, and the other in a clofet, heated as high as could conveniently be fuffered. The operation having been repeated feveral times without any effential difference in the refults, this general conclufion is drawn, that between the temperatures of melting ice and boiling water, the expanfion of

the

the quickfilver is exactly fix French lines, or .532875 decimal parts of an Englifh inch. But it is to be obferved, that the barometer ftood then at 28.77525; whereas, if it had ftood at 30 inches, it would have been .555556, becaufe the expanfion is in proportion to the length of the column. Farther, the interval between the freezing and boiling points in all thermometers, varies with the height of the barometer, or weight of the atmofphere; and it is the cuftom in England to make thermometers when the barometer ftands at 30 inches; that is to fay, 1.225 or 13.8 French lines, higher than when Mr. DE LUC's boiling point was fixed: and fince from his experiments it appears, that each line of additional height in the barometer, raifes the boiling point $\frac{1}{1134}$th part of the interval between that and freezing, it follows that $\frac{180}{1134} = 0.158 \times 13.8 = 2°.2$, will denote the number of degrees, that Mr. DE LUC's boiling point is lower than that of Englifh thermometers, which reduces it to 209.8 of FAHRENHEIT, and makes the interval between freezing and boiling only 177.8 degrees. Hence the expanfion .555556, formerly found, muft be increafed in the proportion of 177.8 to 180, which gives for the total .5624297 or .56243, on a difference of temperature of 180°. Thus the expanfion for each degree, fuppofing it

to be arithmetical, or uniformly the fame in all parts of the fcale, will be .0031 2461 [a].

Having now fhewn the expanfion of quickfilver in the tubes of barometers refulting from the Geneva obferva-tions, I fhall next proceed to give fome account of thofe I made for that purpofe. They derive their origin from my having very accidentally obferved, that a fmall de-gree of heat, and of fhort duration, fenfibly affected the length of the column in Mr. RAMSDEN's portable baro-meter, whereof a view is given in plate XVI. The principal parts of this inftrument are a fimple ftraight tube, fixed into a wooden ciftern, which, for the conve-niency of carrying, is fhut with an ivory fcrew, and, that being removed, is open when in ufe. Fronting this aper-ture is diftinctly feen, the coincidence of the gage-mark, with a line on the rod of an ivory float, fwimming on the furface of the quickfilver, which is raifed or depreffed by a brafs fcrew at the bottom of the ciftern. From this, as a fixed point, the height of the column is readily mea-fured on the fcale attached to the frame, always to $\frac{1}{500}$

(*a*) This paper having lately been communicated to Mr. DE LUC, he hath informed me, that the difference of temperature in his experiments, amounted to about 31° of REAUMUR, or 72° of FAHRENHEIT, above freezing: where-fore, .0031 2461 × 72 = .225 nearly, will denote the rate of expanfion from which he deduced that for 180°; and within thefe limits, it will hereafter be found to differ very little from the refult of the prefent experiments.

I   part
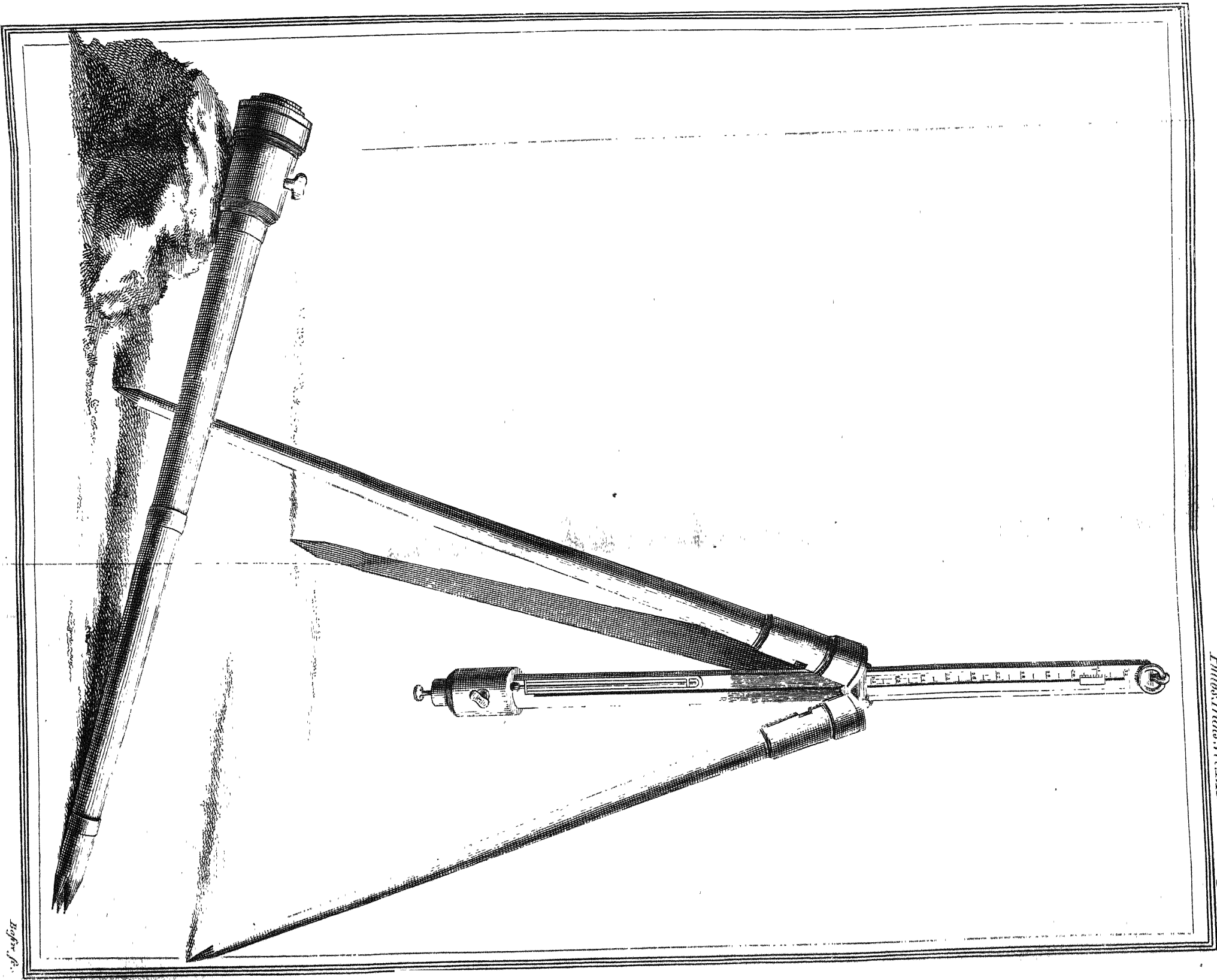
part of an inch, by means of a nonius moved with rack-work. A thermometer is placed near the ciftern, whofe ball heretofore, was ufually inclofed within the wood work, a defect that hath been fince remedied. The three-legged ftand, fupporting the inftrument when in ufe, ferves as a cafe for it, when inverted and carried from place to place. Two of thefe barometers, after the quick-filver in them hath been carefully boiled, being fuffered to remain long enough in the fame fituation, to acquire the fame temperature, ufually agree in height, or rarely differ from each other more than a few thoufandth parts of an inch, which were conftantly allowed for in calcu-lating altitudes, as well as in eftimating the rate of expan-fion, in the courfe of the following experiments.

S E C T I O N   I.

Experiments on the expanfion of quickfilver.

THE experiments made for this purpofe were nu-merous as well as various, and were therefore fubdivided into feveral claffes. To give a minute detail of them all, would be extremely tedious, and now wholly ufelefs, fince it was from thofe of the third clafs alone, that the

rate

rate as well as maximum of expansion was ascertained: wherefore those of the two preceding classes need only be mentioned in a general way.

The first set of the first class comprehended such as were made with one barometer in a cold room, or in the open air, and the other in a room on the same level with the former, where there was constantly a fire, which was occasionally increased, in order to augment the difference of temperature. When the heated barometer had remained several hours in an angle of the room, the difference of temperature of its quicksilver above that of the coldest, as indicated by their respective attached thermometers, rarely exceeded 10 or 12°, which, from a mean of many observations, gave an expansion of .0333 decimals of an inch, for the 10° comprehended between 32 and 42° of FAHRENHEIT's thermometer. So far the result arising in this way, from small differences of temperature, will be found to agree with the third class of experiments.

But when, in the second set of this first class, the difference of temperature was augmented to 20 or 30°, by exposing the barometer within doors to a greater heat, or placing the superior one on the leads, whereby it received the direct and reflected rays of the Sun throughout the greatest part of the day, while the other was kept

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in the cold area underneath, the rate of expanfion for the firft 10° exceeded that formerly found nearly in the proportion of three to two, while that for the fecond and third terms, of 10° each, diminifhed progreffively.

The chief, though not the only caufe of this great difference, as will appear hereafter, arofe from the pofition of the ball of the thermometer, originally inclofed within the wood-work of the frame, which prevented it from receiving the heat fo readily as the quickfilver in the tube; at the fame time that it retained it longer, and confequently produced refults in fome degree fallacious.

Finding, from the firft clafs of experiments, that much uncertainty remained with regard to the rate of expanfion of quickfilver affected by thefe fmaller degrees of heat, and that it was utterly impoffible, from them, to determine its maximum for the 180° between freezing and boiling; I refolved to try, how much a column of 30 inches of quickfilver, carefully boiled in a tube, would lengthen, the fame being placed with the open end upwards in a tin veffel, occafionally filled with pounded ice and water, and afterwards brought to boil, by means of a charcoal fire placed underneath? In this fecond clafs, it was eafy to fee, that the expanfion of the tube containing the quickfilver, was neceffarily to be taken into the account, and added to that apparently found by experi-

2   ment.

ment. This was of courfe to be done, either by fuch dif-
ferences as I could difcern and meafure, or by thofe that
had refulted from the experience of others.

The nature of the apparatus, employed in this clafs of
experiments, will be eafily underftood from plate XVII.
where it is reprefented, as it was ufed in thofe of the third
clafs. In its firft ftate it was not quite fo long, and a
chafing-difh with a charcoal fire, occupied the place of
the ciftern holding the quickfilver below. By means of
a circular bit of tin, foldered edgeways in the center of
the bottom, and an aperture in the middle of the lid, the
tube was kept fteadily in the axis of the veffel. Other open-
ings in the lid, ferved for the admiffion of the thermo-
meter, and the application of a deal rod clofe to the fide
of the tube, when its height was to be meafured. The
longitudinal expanfion of the glafs was marked by a
fcratch thereon with a fine edged file at the top of the
deal rod, when refpectively at the temperatures of freez-
ing and boiling. The apparent dilatation of the quick-
filver was in like manner marked, by the coincidence of
its furface with the lower edge of a brafs ring embracing
the tube.

It having been found impoffible to procure tubes
whofe bores were truly cylindrical, or of any uniform
figure, the experiment was repeated, as often as poffible,

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in both ends of the fame tube, that the mean might be
taken. But it frequently happened that the tube, which
had undergone one or more experiments in one end,
broke before any could be made with it in the other. In
this cafe, the rate of expanfion in the laft end was taken
from that given by fuch another tube, where it had fuc-
ceeded in both. The mean of five refults with the beft
tubes, taken in this way, gave .4901 for the apparent
expanfion of 30 inches of quickfilver, on 180° of FAH-
RENHEIT, between freezing and boiling, which being
augmented by the apparent longitudinal dilatation of the
glafs .0356 × 3 = .1068, the real expanfion is .5969; ex-
ceeding Mr. DE LUC's by more than $\frac{3}{100}$ths of an inch. If,
however, Mr. SMEATON's dilatation of glafs, (.025 × 3 =
.075) be fubftituted, inftead of that refulting from thefe
experiments, the real expanfion of 30 inches of quick-
filver will be .5651, which does not exceed it quite $\frac{3}{1000}$
parts of an inch.

In this clafs of experiments, having attended as dili-
gently as poffible to all the circumftances, it feemed to
me, that tubes with a fmall bore, and whofe glafs was
thick, lengthened more than thofe, which had a larger
bore and whofe glafs was thin: whence I was led to
fuppofe, that folid glafs rods would dilate more in pro-
portion, and confequently, fhew a ftill more perceptible
difference.

difference. With the view of afcertaining this point, I procured four glafs rods near three feet long each, and of different diameters, the largeft being of the fize of the little finger, and the fmalleft about the thicknefs of a quill. One end of each, was fomewhat larger than the other, and was made perfectly fmooth, as that on which they were to reft when feverally meafured with the deal rod. They were then all placed in the tin veffel, in fuch a manner, as to admit pounded ice rammed very clofe around them, and the interftices to be filled with water. Having remained in that ftate a full half hour, they were feverally meafured with the deal rod, whofe length of $32\frac{1}{2}$ inches was fcratched on each with the fharp edge of the file. This being done, the ice thrown out, and the veffel carefully wafhed, all the rods were replaced in it, immerfed in water, which afterwards was brought to boil. The fire being kept up, and the ebullition rendered as violent as poffible for half an hour, the glafs rods were then feverally meafured, by applying them one after another to the deal rod, ftanding with them in the boiling water. The experiment was repeated three times, on as many different days, without its being poffible to difcern, that any of the glafs rods had dilated more than that of deal, from a difference of temperature of 180°. In all of them, the freezing mark feemed accurately to coincide with the top

of

of the deal rod; whereas the dilatation of the tubes, by the fame degree of heat, was always very vifible [b].

Finding from the fecond clafs of experiments, whereof the general refult hath now been given, that glafs rods feemed not to lengthen more than deal; and that tubes of different bores, and probably too of different forts of glafs, were fufceptible of different degrees of extenfion, which rendered it impoffible, by this means, to afcertain the longitudinal expanfion of the quickfilver they contained; I thought it neceffary to recur once more to the barometer, and to try whether it could not be fo contrived as to act in water of different temperatures, from freezing to boiling. This led me to the experiments of the third clafs: and in order to comprehend them thoroughly, it feems neceffary to point out fome few alterations which the apparatus underwent.

The center of the bottom being pierced on purpofe, a brafs focket was prepared for it, wherein a hole was bored conically, to receive the ground-end of a barometer tube, of the ordinary length of $33\frac{1}{2}$ inches; the tube having been firft ground in a feparate piece of brafs, and ulti-

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(b) Since thefe experiments were made, the relative expanfion of 18 inches of one of the tubes and one of the rods that had been formerly employed, was found to be, by Mr. CUMMING's pyrometer, nearly as 4 to 1, from a heat approaching to that of boiling oil.

mately

mately in the focket itfelf, fitted it fo exactly, as to fuffer no water to pafs. The focket being inferted into the aperture at the bottom of the veffel, was firmly foldered to it for the reception of the tube, which was fo ground as to reach a full inch and a half below the furface of the brafs. It could not defcend farther, the ground parts in both being of the figure of the fruftum of an inverted cone. From the view in the plate it will appear, that underneath the veffel, a feparate ftand was placed, in order to fupport the iron ciftern containing the quickfilver. The diameter of the ciftern was fuch, that its ftand being occafionally moved, fo as to bring one fide of it clofe to the ground part of the tube, the other fide projected beyond the bottom of the veffel; and confequently permitted the rod of a float, refting on the furface of the quickfilver, to rife freely and parallel to the axis of the tube. The rod was of deal, $\frac{1}{10}$th of an inch fquare, carrying on its top a fcale, whofe zero lay in the lower furface of the float; and whereof the fix uppermoft inches, from 28 to 34, were divided into 20ths.

That the whole column of quickfilver might alternately be covered with the freezing mixture and water of different temperatures, and yet permit its furface to be feen, two eyes of plate glafs were fcrewed into fockets, foldered for that purpofe oppofite to each other, near the

top.

top of the veffel, which, in the firft fet of the third clafs of experiments, was little more than 29 inches high. The top of the tube paffing through the aperture in the lid, one and a half or two inches of the vacuum generally rofe above the veffel. That the expanfion of the column might be meafured as nearly as poffible in that part of the tube fronting the center of the eyes, more or lefs quickfilver, according to the ftate of the atmofphere, was occafionally put into the ciftern, to raife or deprefs the furface of the column to the proper height. A thin brafs ring, whofe lower parts were made to fpring, embraced with fufficient force the upper part of the tube, permitting it at the fame time to be moved freely with the hand. It carried along with it a nonius index, projecting as far as the center of the rod, and confequently applying itfelf to the divifions of the fcale, which was kept in its proper pofition by paffing through a flit fitted for it in an arm attached to the lid. The divifions on the nonius being the fame with thofe of the barometer formerly defcribed, the height of the quickfilver could always be read off to $\frac{1}{500}$th part of an inch.

The quickfilver having been carefully boiled, as on former occafions, in the tube; and that being filled completely, and held with its open end upwards; the tin veffel was inverted over it, and lowered gradually, till the

ground

ground end could be inferted into the focket with fuch a degree of force as to prevent it from being too eafily removed. The finger being then applied clofely to the open end of the tube, the whole apparatus was turned up, and placed over the ciftern into which the quickfilver had previoufly been put, great care being taken not to remove the finger till the lower extremity of the tube was fairly immerfed into the quickfilver; when that in the tube was permitted to defcend into an equilibrium with the atmofphere. In the firft experiment it was found that the water iffued by the eyes, and running down the fide of the veffel, fell into the ciftern. In order to remedy this inconveniency, a circular piece of tin was foldered round the upper part of it, immediately below the eyes; and a flat fpout, projecting from it, ferved as a gutter to throw off the water from the ciftern, and from the lamps made ufe of to bring that in the veffel to boil. Six lamps, each with a double light, were fufpended around the trunk of the veffel, to heat the water as equally as poffible; though any irregularity of this kind was fufficiently guarded againft, by conftantly mixing it during the operation. Another lamp of the fame kind ftood under the ciftern, whereby the quickfilver there was kept at the temperature of the water in the veffel, each having its proper thermometer: this laft lamp was placed

and

and difplaced frequently, during the courfe of every experiment; for the heat was very expeditioufly communicated to the iron ciftern, and thence to the quickfilver it contained; and both were found to cool very faft, after the lamp was removed. Such was the ftate of the apparatus, when the firft fet of this third clafs of experiments was made. In thofe of the fecond fet, its height was farther augmented by tin foldered to the top, that a tube of the ordinary length might be wholly immerfed in boiling water. The third and laft alteration confifted in the occafional application of a detached tin cafe, equal in diameter to the upper part of the veffel, having a hole in its bottom to admit the top of a long tube to pafs. This cafe was fo contrived, that its bottom ftood two inches and a half higher than the lid of the veffel, thereby allowing room for the hand to move the index up or down. In this ftate the apparatus is reprefented in the view; and its various ufes will be beft underftood from the account of the experiments, which were fubdivided into four fets.

Thofe of the firft fet were made with tubes of a large bore, upwards of three-tenths of an inch in diameter, of the ordinary length, with a vacuum over the quickfilver of two inches and a half or three inches, part of which reached above the top of the veffel. The mean of three

experi-

experiments gave .5258, for the total dilatation of 30 inches of quickſilver, on 180° between freezing and boiling; that, anſwering to the firſt 20°, between 32° and 52°, was .0688; that, for the 20° in the middle of the ſcale, between 112° and 132°, was .058; and the rate for the laſt 20°, between 192° and 212°, was only .041. From this firſt ſet of the third claſs of experiments, it appeared evident, that the expanſion of 30 inches of quickſilver in the barometer, ſuffering a heat equal to 180° of FAHRENHEIT, inſtead of exceeding Mr. DE LUC's, as appeared to be the caſe from the reſults of the open tube, really fell ſhort of it: and inſtead of being arithmetical or uniformly the ſame, for equal changes of temperature, was actually progreſſive; the expanſion anſwering to the lower part of the ſcale, being greater than that correſponding to the middle; which again exceeded that for high temperatures. In theſe experiments, when the water had acquired a heat 20 or 30 degrees greater than that of the open air, a certain duſtineſs was perceived in the vacuum of the tube. At 100° of FAHRENHEIT, or thereabout, this appearance had ſo far increaſed, as to ſhew clearly, that it could proceed from no other cauſe than a vapour ariſing from the ſurface of the heated quickſilver, quite inviſible, till, by its condenſation in the cold part of the tube, it was formed into balls, every where adhering to

its

its fides and fummit. Thefe globules were very fmall near the furface of the water, augmenting gradually as they approached the top of the tube, where they were greateft: their bulk increafed with the heat; and when the water was at or near boiling, they would fometimes unite, and defcend by their own gravity, along the fides of the tube, into the general mafs. Hence the progreffive diminution of the rate of expanfion of the column of quickfilver in the barometer, perceptible even in the firft clafs of experiments, is eafily accounted for by the refiftance of the elaftic vapour[c], acting againft the top of the tube, which was here colder than the reft.

But in the application of the barometer to the meafurement of heights, the whole inftrument is of the fame temperature; wherefore, in the fecond fet of this third clafs of experiments, the tin veffel was heightened, that tubes of the ordinary length, placed in it, might be wholly immerfed in boiling water. The mean of four experiments, which agreed very nearly among themfelves, gave .5117 for the total expanfion between freezing and boiling; for the 20°, between 112° and 132°.059; and for the laft 20°, between 192° and 212°.046. In thefe

(c) Having mentioned the circumftances to Mr. RAMSDEN, it firft occurred to him, that the refiftance of the elaftic vapour was the caufe of the diminution in the rate of expanfion.

experi

experiments, the tube being wholly covered with boiling water, no condenſation of vapour took place in the vacuum; and therefore no particles of quicꝭ ſilver were ſeen adhering to the upper part of the tube. When the water boiled, the reſiſtance of the vapour was greater than in the preceding ſet, and the total expanſion leſs. Theſe two reſults ſerve ſtrongly to confirm each other: it is, however, the laſt that furniſhes the data for conſtructing the table of equation depending upon the heat of the quickſilver in the barometer, of which table we ſhall give an account hereafter.

Finding, from the compariſon of theſe two ſets of experiments with each other, that the maximum and rate of expanſion ſeemed to vary with the length of the vacuum above the quickſilver, I was adviſed to try[d] what might be the reſult, when the vacuum was much longer than in the common barometer.

The third ſet of experiments of this claſs was therefore made with a tube ſomewhat narrower in the bore than the former, and whoſe vacuum was 14½ inches in length, whereof 11½ reached above the top of the veſſel. The mean of three obſervations gave .5443 for the total expanſion on 180°; that for the firſt 20° was .067; for

(d) Dr. BLAGDEN, who afterwards aſſiſted in ſome of the firſt experiments with the manometer, propoſed that with the long tube.

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the 20° in the middle of the fcale .058; and for the up-
permoft 20°.065: whence the mean rate for every 20°,
is nearly .0605[e]. In this fet, the condenfation in the va-
cuum of the tube was particularly attended to: it began,
as in thofe of the firft fet, immediately above the furface
of the boiling water, which was always kept an inch or
two above the top of the column: the lowermoft glo-
bules were very fmall, increafing gradually till they got
without the lid of the veffel, where they were the largeft;
thence they diminifhed uniformly upwards, and difap-
peared entirely three or four inches below the top of the
tube. Though the rate for the middlemoft 20°, in thefe
laft experiments, be below the mean, probably from fome
inaccuracy in obfervation; yet, being compared with the
former fets, they ftill ferve to corroborate each other: for
in thefe with the long tube, the vacuum feems to have
been either completely maintained, or nearly fo; and we
accordingly find the maximum of expanfion increafed,
and its rate rendered nearly uniform, as will be farther
confirmed from what follows.

(e) Mr. CAVENDISH, who affifted in the firft part of the experiments with
the open tube, informed me, that, in thofe made by his father Lord CHARLES,
the difference between the expanfion of quickfilver and glafs, from 180° of heat,
was .469. If to this we add Mr. SMEATON's dilatation of glafs, the total
expanfion of 30 inches of quickfilver will be .544, which agrees with the
experiments in the long tube, and gives a rate of only .003022 for each degree.

I have already had occaſion to mention that a detached tin caſe was ſometimes applied above the veſſel, in which ſtate it is repreſented in the view. This method was thought of during the operations with the long tube, in order to try whether the vacuum was completely maintained by the temperature of the open air? For this purpoſe the caſe was placed on the ſtones of the yard, with a ſmall tube inſerted in it, to preſerve an open paſſage in the middle: it was then rammed full of a compoſition of ſalt and ice; and afterwards fixed on the top of the long tube. The degree of cold thus applied round the greateſt part of the vacuum, muſt have been very great, probably near the zero of FAHRENHEIT; yet it produced no viſible alteration in the height of the column of quickſilver, which ſtill remained in boiling water below, and ſhould have riſen, if the vacuum had been formerly incompleat. As it would have occaſioned much trouble to have lengthened the ſeveral parts of the apparatus ſo as to have kept the long tube wholly in boiling water, the counterpart of this laſt experiment was not made in the accurate manner it ought: neverthelefs, the tin caſe, being emptied of its cold compoſition, was placed on the tube as before, and filled with boiling water; which, joining with the intermediate ſteam ariſing from that in the veſſel below, muſt have kept the whole nearly in the

fame

fame temperature. The confequence of this application was, that the column fhortened about $\frac{2}{100}$ths of an inch; which feems to prove, that the quickfilver vapour now reached the fummit of the tube, and, acting againft it, overcame, by fo much, the preffure of the atmofphere.

I fhould now proceed to give fome account of the fourth fet of this laft clafs of experiments, made on the condenfation of the quickfilver, by means of artificial cold, below the temperature of the air. Previoufly however to this, it may not be improper to take notice, in a more general way, of fome others that were made on expanfion; as tending, with certain circumftances yet to be mentioned, not only to confirm thofe already defcribed, but likewife to account for many irregularities that occur in operating with barometers.

In the courfe of the preceding experiments, from accidents of various kinds, it was often neceffary to reboil the quickfilver; and in that operation, many tubes were broken. The frequent removal of the focket from the bottom of the veffel, in order to get others ground for it, became at laft very troublefome; and made more caution neceffary, in boiling fuch as were ground, efpecially in frofty weather, which happened to be the cafe in the laft days of March, 1775: wherefore it was thought beft in the interim to try, what might be the expanfion of a column

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of quickfilver, carefully put into the tube, but not boiled
therein?

With this view, the ftandard barometer and apparatus
were left out during the night of the 29th, that they
might acquire the fame temperature, which was found
next morning to be $34°\frac{1}{2}$; the unboiled quickfilver ftand-
ing $\frac{1}{100}$th of an inch higher than that which had been
boiled. The lamps being applied to the veffel, the
lengthening of the unboiled column was perceived, on
the whole, to be more irregular, and the progreffive di-
minution quicker, than in former experiments; fo as to
give, for the maximum of expanfion, only .443 for 180°.

On the morning of the 31ft, the unboiled column,
which on the preceding day had been the higheft, was
lower than the other by near $\frac{2}{100}$ths of an inch, the tempe-
rature of both being $31°\frac{1}{2}$. As the water acquired heat
from the application of the lamps, the rate of expanfion
diminifhed; and, at boiling, was only .405 for 180°. The
operation of the 30th feems to point out, in a manner
fufficiently conclufive, that the air contained in the un-
boiled quickfilver, rendered its fpecific gravity lefs, than
that which had been boiled even a great while before;
fince it required a longer column of the firft, to counter-
balance the weight of the atmofphere. And though the
vacua might poffibly, at the beginning, have been equally

compleat

compleat in both; yet they could not continue long fo: for the air efcaping gradually from the unboiled quick-filver, its elafticity increafing with the heat, and uniting with the quickfilver vapour, muft have refifted the dila-tation of the column, and rendered it lefs than on for-mer occafions; which actually appeared from experi-ment. This is farther confirmed by the obfervations of the fubfequent day; for now the unboiled column was be-come the fhorteft, owing no doubt to more air having afcended, and rendered the vacuum ftill more incom-pleat. Thus, the caufes of refiftance increafing, the di-latation is leffened in a fuperior degree.

The other circumftances to be mentioned, occurred on the 12th of April. After finifhing one of the experiments of the fecond clafs, and when the water had cooled to 192°, the veffel, by accident, received a fudden jolt, whereby the mouth of the tube muft have been raifed, for a mo-ment, out of the quickfilver in the ciftern. In a few mi-nutes after this, intending to obferve how far the column had fhortened from the decreafing heat, I was furprized to find, that the quickfilver had wholly difappeared in the tube, and was funk fo low as not to be feen by looking obliquely down at the eye of the veffel. It was then cer-tain that air, and probably a particle of moifture along with it, had afcended into the upper part of the tube,

whereby

whereby the vacuum was deſtroyed in ſo remarkable a degree. Since this accident made it neceſſary to reboil the quickſilver, the water (then between 180° and 190°) was let out by the cock fixed for that purpoſe at the bottom of the veſſel; but before it could be intirely drawn off, the tube and its contents, had ſo ſenſibly felt the condenſing force of the ſurrounding atmoſphere, then about 48°, that the quickſilver had riſen again, and preſented itſelf oppoſite to the eye of the veſſel, ſomething lower indeed than where it formerly ſtood. On this diſcovery, and as ſoon as water could be boiled for the purpoſe, the veſſel was filled again, when the quickſilver ſubſided, as before, quite out of ſight; and on drawing off the water a ſecond time, it roſe anew, ſeemingly to its former height.

The appearance, which this accidental circumſtance produced, was ſuch, as naturally ſuggeſted that farther experiments might have been made, varied as much as poſſible from each other, by the admiſſion of different quantities of air, or of air and moiſture intermixed. But the nature of the veſſel rendering it impoſſible to ſee, and conſequently to meaſure, the motion of the quickſilver, occaſioned by the alternate expanſion and condenſation of the elaſtic vapour contained in the upper part of the tube, and which could not have been accom-
pliſhed

plifhed without many troublefome alterations in the apparatus, therefore nothing of the kind was attempted. From the circumftances juft now mentioned, it will be readily conceived, how much care is neceffary in operating with barometers for the meafurement of heights, that the vacua be as nearly as poffible compleat; and particularly, that no moifture get up into the tube. I now proceed to the fourth and laft fet of experiments.

Having found, from the two firft fets of this clafs, the rate of expanfion of a column of quickfilver, in the tube of a barometer of the ordinary length, to be progreffive and not arithmetical; and that its maximum, for the 180° comprehended between feeezing and boiling, was lefs than had been fuppofed; I thought it proper to try, by means of artificial cold, whether the condenfation, for the 32° below freezing, followed nearly the fame law?

For this purpofe the tin veffel, containing the ground tube, was rammed quite full of pounded ice and falt, as well as the tin ftand holding the iron ciftern below. In this operation, twelve pounds of ice and four pounds of falt were employed, whereby the mean temperature of the mixture was reduced to + 4° of FAHRENHEIT. But before the eyes of the veffel could be fufficiently freed from the compofition, fo as to permit the furface of the column to be diftinctly feen and read off; it had rifen to

5   + 14°

+14°; the temperature of the air, and also of the standard barometer, being at the same moment 49°$\frac{1}{2}$. The observed condensation, arising from this difference of 35°$\frac{1}{2}$, was $\frac{12}{100}$ths of an inch; or .1189, when reduced for the height of the barometer, which then stood at 30.296. Hence the condensation for 32° is .1072, or .00335 for each degree. In this day's experiment, when the temperature of the mixture had risen to 32°, that of the air and standard barometer was 52°$\frac{1}{4}$; whence the reduced difference, for the 20° between 32° and 52°, was found to be .0664, answerable to former experiments.

The same experiment was repeated two days after, with great care, the vessel being filled no higher than the surface of the quicksilver. The mean temperature of the mixture was now +4°, and that of the standard barometer 49°$\frac{1}{4}$. The observed condensation, arising from this difference of 45°$\frac{1}{4}$, was $\frac{162}{1000}$; or .1594, when reduced for the height of the barometer, then standing at 30,416: hence the rate for 32° is .1127, or .003,522 for each degree. When the temperature of the mixture had risen to 32°, that of the air was 51°: whence the augmented rate for the 20°, between 32° and 52°, was found to be .0662.

From the mean of these two experiments it appears, that the condensation of a column of 30 inches of
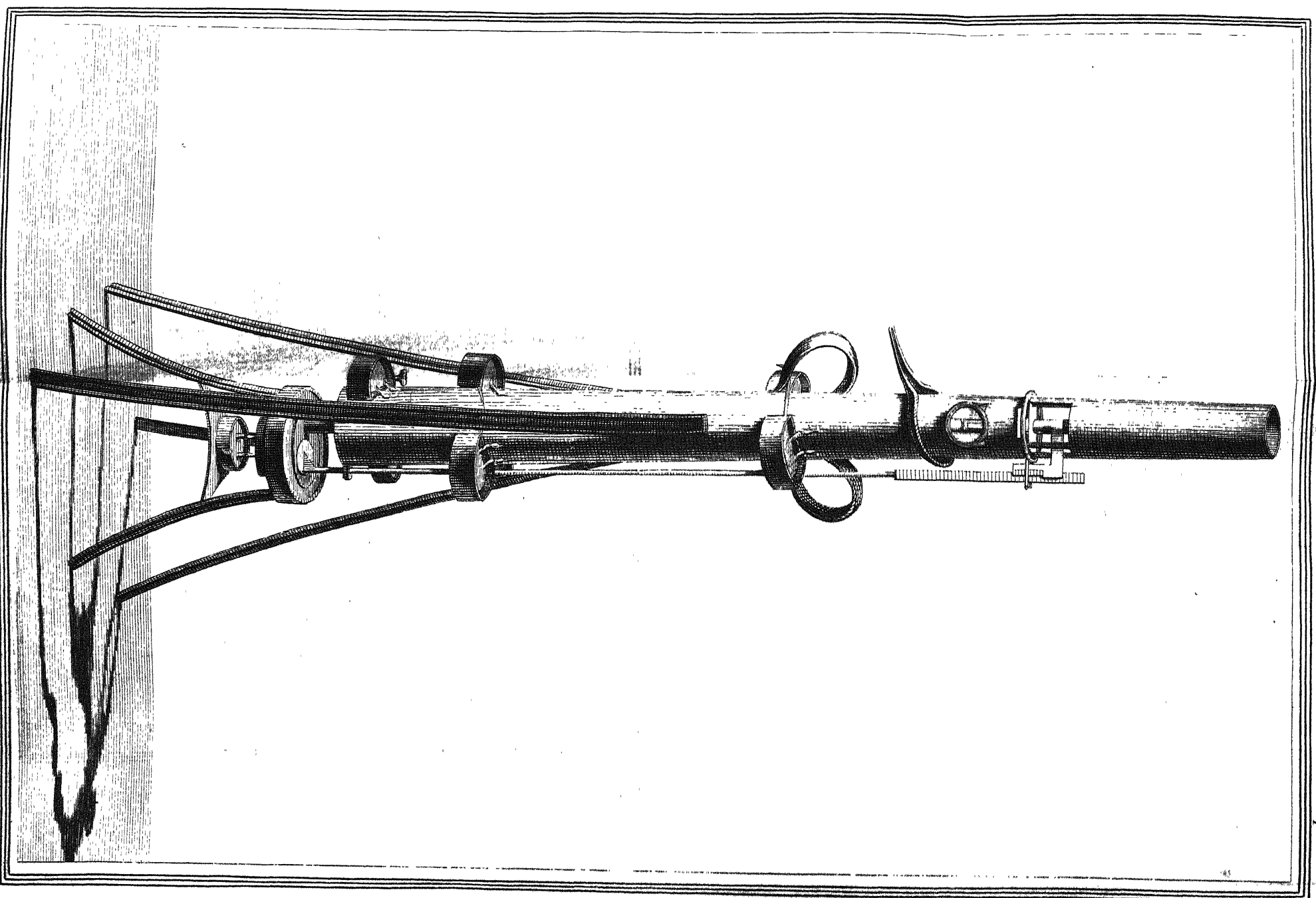
quickfilver in the barometer, affected by the 32° of cold below freezing, is .1099: and that the expanfion from 20° of heat, between 32° and 52°, is .0663, a number agreeing perfectly well with former refults. If the con-denfation .1099 thus found, be added to the expanfion .5117 arifing from the fecond clafs of experiments, we fhall have .6216 for the total difference of height of the columns of quickfilver in two barometers, fuftaining the fame preffure, but differing from each other in their tem-peratures 212° of FAHRENHEIT's thermometer.

The feries of numbers expreffed in the annexed table, agreeing in all effential refpects with the expanfions found by experiment, will therefore fhew that which correfponds to any intermediate temperature, for every 10° of the fcale.

Rate of expanfion of a column of quickfilver in the tube of a barometer.

	Temperature.	Expanfions.	Differences.	2d Differences.
Expanfion above 32° of FAHRENHEIT; *equation to be fubtracted from the height of the column of quickfilver of 30 inches.*	212	.5117		
			.0229	
	202	.4888		
			.0236	
	192	.4652		
			.0243	
	182	.4409		.0007
			.0250	
	172	.4159		
			.0257	
	162	.3902		
			.0264	
	152	.3638		
			.0271	
	142	.3367		
			.0277	
	132	.3090		
			.0283	
	122	.2807		
			.0289	.0006
	112	.2518		
			.0295	
	102	.2223		
			.0301	
	92	.1922		
			.0307	
	82	.1615		
			.0313	
	72	.1302		
			.0318	
	62	.0984		
			.0323	
	52	.0661		
			.0328	
	42	.0333		
			.0333	.0005
Condenfation below 32° of FAHRENHEIT; *equation to be added.*	32	.0000		
			.0338	
	22	.0338		
			.0343	
	12	.0681		
			.0348	
	2	.1029		
			.0070	
	0	.1099		

*Conftruction and application of the table of equation, for
the expanfion of the quickfilver in the tubes of baro-
meters.*

In the introduction to this paper there was occafion to
remark, that in the application of the barometer to the
meafurement of heights, various modes of calculation
had been adopted. The eafieft and beft method feems
however to be, by means of the tables of common loga-
rithms, which were firft thought of by Mr. MARIOTTE,
and afterwards applied by Dr. HALLEY, Mr. BOUGUER,
Mr. DE LUC, and others. They have all proceeded on
the fuppofition, that air is a truly homogeneous and elaf-
tic fluid, whofe condenfations being proportionable to
the weights with which it is loaded, its dilatations are in
the inverfe of the weights; and in confequence of this
law, that the heights of the atmofphere afcended, are in
geometrical progreffion, while the correfponding fuc-
ceffive defcents of the quickfilver in the tube of the
barometer, are in arithmetical progreffion.

Mr. DE LUC makes ufe of an arithmetical or uniform
equation for the heat of the quickfilver in his barome-
ters, whereby their relative heights are reduced to what
they would have been in the fixed temperature of $54°\frac{1}{4}$

of

of FAHRENHEIT. In the formulæ adapting his rule to Englifh meafures (Phil. Tranf. vol. LXVII. N° xx. and xxx.) hath been fhewn, that the eafieft and fimpleft method is, to make the difference of temperature of the two barometers the argument for the equation; and that it is fufficient to reduce either column to what would have been its height in the temperature of the other. But whatever may heretofore have been the method of ufing the equation for the heat of the quickfilver, while it was confidered as arithmetical; now that it hath been fhewn, from the preceding experiments, to be progreffive, there feems at leaft to be propriety in applying to each barometer the equation anfwering to its particular temperature. And though, for this purpofe, any fixed temperature might have been affumed at pleafure, as that to which both barometers were to be reduced; yet, the freezing point being fundamental in all thermometers, and that being likewife the zero of the fcale for the equation depending on the heat of the air, as will be fhewn hereafter, it hath been preferred to any other.

From the experiments it appears, that a column of quickfilver of the temperature of 32°, fuftained, by the weight of the atmofphere, to the height of 30 inches in the barometer, when gradually affected by different degrees of heat, fuffers a progreffive expanfion; and that,

having

having acquired the heat of boiling water, it is lengthened $\frac{5117}{10000}$ parts of an inch : alfo, that the fame column, fuffering a condenfation by 32° of cold, extending to the zero of FAHRENHEIT, is fhortened $\frac{1099}{10000}$ parts, the weight of the atmofphere remaining in both cafes unaltered; but that, in the application of the barometer to the meafurement of altitudes, fince the preffure and length of the column change with every alteration of vertical height, the equation, depending on the difference of temperature of the quickfilver, will neceffarily augment or diminifh by a proportionable part of the whole. Thus, if the weight of the atmofphere fhould at any time be fo great as to fuftain 31 inches of quickfilver, the equation for difference of temperature will be juft $\frac{1}{30}$th part more than that for 30 inches; at 25 inches it will be $\frac{5}{6}$ths; at 20 inches $\frac{2}{3}$ds; at 15 inches $\frac{1}{2}$; and at 10 inches only $\frac{1}{3}$d of that deduced from experiment.

It is upon thefe principles that the annexed table of equation hath been conftructed, for differences of temperature extending to 102° of the thermometer, and for heights of the barometer from 15 to 31 inches; beyond which limits, it is not probable, that many barometrical obfervations will be made. The firft or left-hand column, fhews the height of the barometer for every half inch, from 31 to 25; thence for every inch downwards

to

to 20; the 15th inch being half of the obferved expanfion. The five next columns towards the right, comprehend the additive equation for condenfations, anfwering to 0°, 12°, and 22°, with their intermediate differences; thofe that are progreffive, as arifing from difference of temperature, being ranged horizontally; and thofe that are arithmetical, as depending on the height of the barometer, being placed vertically. The temperature of 32° requires no equation, and the thirteen columns from thence towards the right hand, contain the fubtractive equations for expanfion, correfponding to every 10° as far as 102°, with their progreffive and arithmetical differences ranged as before. By means of thefe differences, the equation for intermediate temperatures may readily be taken out by infpection. Hence is deduced the firft part of the rule for meafuring heights by the barometer. When the temperature of the quickfilver is below 32° of FAHRENHEIT, add the correfponding equation for condenfation to the obferved heights of the columns refpectively; when above 32°, fubtract the equation for expanfion from the obferved heights of the columns refpectively; with which equated heights of quickfilver, expreffed in 1000th parts of an inch, the tables of logarithms are to be entered.

Table

Table, shewing the equation to be applied to the observed height of quicksilver in the barometer, from 15 to 31 inches; and for differences of temperature extending to 102° of FAHRENHEIT: whereby the column is reduced to the height it would have stood at in the temperature of 32°.

Observed height of quicksilver in the Barom.	Condensation below 32°; Equation to be added to the height of the quicksilver in the barometer.					32"	Expansion above 32°; Equation to be subtracted from the height of the quicksilver in the Barometer.											
	0°	Diff.	12°	Diff.	22°		42°	Diff.	52°	Diff.	62°	Diff.	72°	Diff.	82°	Diff.	92°	Diff.	102°
31	.1136	.0432	.0704	.0355	.0349		.0344	.0339	.0685	.0334	.1017	.0328	.1345	.0324	.1669	.0317	.1986	.0311	.2297
30½	.1118	.0425	.0693	.0349	.0344		.0339	.0333	.0672	.0328	.1000	.0323	.1323	.0319	.1642	.0312	.1954	.0306	.2260
30	.1099	.0418	.0681	.0343	.0338		.0333	.0328	.0661	.0323	.0984	.0318	.1302	.0314	.1616	.0306	.1922	.0301	.2223
29½	.1081	.0411	.0670	.0338	.0332		.0327	.0323	.0650	.0318	.0968	.0312	.1280	.0309	.1589	.0301	.1890	.0296	.2186
29	.1063	.0405	.0658	.0331	.0327		.0322	.0317	.0639	.0312	.0951	.0307	.1258	.0304	.1562	.0296	.1858	.0291	.2149
28½	.1045	.0398	.0647	.0326	.0321		.0316	.0312	.0628	.0307	.0935	.0302	.1237	.0298	.1535	.0291	.1826	.0286	.2112
28	.1026	.0390	.0636	.0321	.0315		.0311	.0306	.0617	.0301	.0918	.0297	.1215	.0293	.1508	.0286	.1794	.0281	.2075
27½	.1008	.0384	.0624	.0314	.0310		.0305	.0301	.0606	.0296	.0902	.0291	.1193	.0288	.1481	.0281	.1762	.0276	.2038
27	.0990	.0377	.0613	.0309	.0304		.0300	.0295	.0595	.0291	.0886	.0285	.1171	.0283	.1454	.0276	.1730	.0271	.2001
26½	.0971	.0370	.0601	.0302	.0299		.0294	.0290	.0584	.0285	.0869	.0281	.1150	.0277	.1427	.0271	.1698	.0266	.1964
26	.0953	.0363	.0590	.0297	.0293		.0289	.0284	.0573	.0280	.0853	.0275	.1128	.0272	.1400	.0266	.1666	.0261	.1927
25½	.0935	.0356	.0579	.0292	.0287		.0283	.0279	.0562	.0275	.0837	.0269	.1106	.0267	.1373	.0261	.1634	.0256	.1890
25	.0916	.0349	.0567	.0285	.0282		.0278	.0273	.0551	.0269	.0820	.0265	.1085	.0261	.1346	.0256	.1602	.0251	1853
24	.0880	.0336	.0544	.0273	.0271		.0266	.0263	.0529	.0258	.0787	.0254	.1041	.0252	.1293	.0245	.1538	.0240	.1778
23	.0843	.0321	.0522	.0263	.0259		.0255	.0252	.0507	.0248	.0755	.0243	.0998	.0241	.1239	.0235	.1474	.0230	.1704
22	.0807	.0408	.0399	.0251	.0248		.0244	.0241	.0485	.0237	.0722	.0232	.0954	.0231	.1185	.0225	.1410	.0220	.1630
21	.0770	.0294	.0476	.0239	.0237		.0233	.0230	.0463	.0226	.0689	.0222	.0911	.0220	.1131	.0215	.1346	.0210	.1556
20	.0733	.0280	.0453	.0228	.0225		.0221	.0220	.0441	.0215	.0656	.0211	.0867	.0210	.1077	.0204	.1281	.0201	.1482
15	.0550	.0211	.0339	.0170	.0169		.0166	.0165	.0331	.0161	.0492	.0158	.0650	.0158	.0808	.0153	.0961	.0150	.1111

(Centre column, vertical text) When the temperature of the quicksilver in the barometer is 32° of FAHRENHEIT, no equation is necessary.

(Bracket difference values, reading across: .0183, .00114, .000563, .000555, .001102, .00164, .00217, .002692, .003203, .003705; lower group: .00366, .00228, .001126, .001110, .002204, .002102, .00328, .00434, .005384, .006406, .007410; bottom group: .0183, .0114, .0563, .0555, .01102, .0164, .0217, .02692, .03203, .03705)

SECTION

SECTION II.

Experiments on the expanfion of air in the Manometer.

WITH refpect to order of time, the manometrical experiments were made fubfequently to the chief part of the barometrical obfervations, from which alone an approximate rule had previoufly been deduced for the meafurement of heights: neverthelefs, in this paper it feemed to me beft, that what related to the expanfion of air in one inftrument, fhould immediately fucceed the expanfion of quickfilver in the other.

The thermometer made ufe of in thefe experiments is above four feet long. Its fcale extends from − 4° to + 224° of FAHRENHEIT, each degree being more than $\frac{2}{10}$ths of an inch: when the barometer ftood at 30 inches, its boiling point was fixed in the tin veffel formerly defcribed. Mr. RAMSDEN's thermometers generally rife in the fame veffel 2 1 3°$\frac{1}{2}$; and the long thermometer, being placed in the veffel he makes ufe of to fix his boiling points, rifes only to 2 1·0°.

The manometers were of various lengths; from four to upwards of eight feet: they confifted of ftraight tubes,

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whofe

whofe bores were commonly from $\frac{1}{15}$th to $\frac{1}{25}$th of an inch in diameter. The capacity of the tube was carefully meafured, by making a column of quickfilver, about three or four inches in length, move along it from one end to the other. Thefe fpaces were feverally marked, with a fine edged file, on the tubes; and transferred from them to long flips of pafte-board, for the fubfequent conftruction of the fcales refpectively belonging to each. The bulb, attached to one end of the manometer at the glafs houfe, was of the form of a pear, whofe point being occafionally opened, dry or moift air could be readily admitted, and the bulb fealed again, without any fenfible alteration in its capacity.

The air was confined by means of a column of quickfilver, long or fhort, and with the bulb downwards or upwards, according to the nature of the propofed experiment. Here it muft be obferved that, from the adhefion of the quickfilver to the tube, the inftrument will not act truly, except it be in a vertical pofition; and even then, it is neceffary to give it a fmall degree of motion, to bring the quickfilver into its true place; where it will remain in equilibrio, between the exterior preffure of the atmofphere on one fide, and the interior elaftic force of the confined air on the other.

All

All the experiments were made when the barometer was at, or near, 30 inches. When the bulb was downwards, the height of the barometer at the time of obfervation, augmented, and when upwards, diminifhed by the number of inches of quickfilver in the tube of the manometer, expreffed the denfity of the confined air.

Pounded ice and water were ufed to fix a freezing point on the tube; and by means of falt and ice, the air was farther condenfed, generally four, and fometimes five or fix degrees below zero. The thermometer and manometer were then placed in the tin veffel, among water which was brought into violent ebullition; where having remained a fufficient time, and motion being given to the manometer, a boiling point was marked thereon. After this the fire was removed, and the gradual defcents of the piece of quickfilver, correfponding to every 20 degrees of change of temperature in the thermometer, were fucceffively marked on a deal rod applied to the manometer. It is to be obferved, that both inftruments, while in the water, were in circumftances perfectly fimilar; that is to fay, the ball and bulb were at the bottom of the veffel.

In order to be certain that no air had efcaped by the fide of the quickfilver during the operation, the manometer was frequently placed a fecond time in melting ice.

ice. If the barometer had not altered between the be-
ginning and end of the experiment, the quickfilver al-
ways became ftationary at or near the firft mark. If any
fudden change had taken place in the weight of the at-
mofphere during that interval, the fame was noted, and
allowance made for it in afterwards proportioning the
fpaces.

Long tubes, with bores truly cylindrical or of any
uniform figure, are fcarcely ever met with. Such how-
ever as were ufed in thefe experiments, generally tapered
in a pretty regular manner from one end to the other.
When the bulb was downwards, and the tube narrowed
that way, the column of quickfilver confining the air
lengthened in the lower half of the fcale, and aug-
mented the preffure above the mean. In the upper half,
the column being fhortened, the preffure was diminifhed.
below the mean. In this cafe, the obferved fpaces both,
ways from the center, were diminifhed in the inverfe.
ratio of the heights of the barometer at each fpace, com-
pared with its mean height. If the bore widened to-
wards the bulb when downwards, the obferved fpaces,
each way from the center, were augmented in the fame.
inverfe ratio; but in the experiments on air lefs denfe
than the atmofphere, the bulb being upwards, the fame
equation was applied with contrary figns: and if any

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extraordinary irregularity took place in the tube, the cor-
refponding fpaces were proportioned both ways from
that point, whether high or low, that anfwered to the
mean.

The obferved and equated manometrical fpaces being
thus laid down on the pafte-board containing the mea-
fures of the tube; the 212° of the thermometer, in exact
proportion to the fections of the bore, were conftructed
along-fide of them: hence the coincidences with each
other were eafily feen; and the number of thermome-
trical degrees anfwering to each manometrical fpace,
readily transferred into a table prepared for the purpofe.

I have already had occafion to remark that, from the
operations of the barometer alone, an approximate rule,
or mean equation, had been obtained for the meafure-
ment of heights; but as, among the refults, irregularities
were now and then met with, doubts naturally arofe,
whether the equation, inftead of being confidered as uni-
form, might not follow an increafing or diminifhing pro-
greffion? Without an infinite number of obfervations,
in very different temperatures above and below the zero
of the fcale, this point could not poffibly be determined
by the barometer: wherefore the firft and chief thing
propofed to be difcovered by the manometrical experi-
ments was, whether common air, occafionally rendered

more

more or lefs denfe, by the addition or fubtraction of
weight, expanded equally with quickfilver, when af-
fected with the fame degrees of heat? According to the
ratio that took place between the expanfion of quickfil-
ver and air, above and below zero, I intended to regulate
the barometrical equation already found, without re-
garding the proportion of the bulb to the bore of the
manometer; or in other words, without paying any at-
tention to the actual expanfion of the air confined in that
inftrument.

But after a great number of thefe firft experiments
had been made, it was judged proper to compute the
actual expanfion of 1000 equal parts of air in the ma-
nometer, from a heat of 212°; wherefore, in the laft, the
accurate capacity of the bulb, with refpect to the bore,
was determined; at the fame time that the original mode
of comparing the thermometrical with the manometrical
fpaces, was ftill adhered to.

It is eafy to conceive in experiments of this very de-
licate nature, part of which, namely thofe on air lefs
denfe than the atmofphere, were extremely difficult and
even laborious, that mathematical exactnefs was not to be
looked for; and that, notwithftanding every poffible pre-
caution was taken, irregularities would occur. Thefe,
however, were not fo numerous as might have been

I   expected,

expected, nor any way fo great as to render the refearch
fruitlefs: for a few of that kind being thrown out of the
total number, the mean of the others, which were very
confiftent among themfelves, ferved to prove beyond the
poffibility of doubt, that the expanfions of common air
did not keep pace with the dilatations of quickfilver.
The manometrical fpace, anfwering to the 20° of the
thermometer between 52° and 72°, was always found to
be greater than any other 20° of the fcale. Here it is to
be underftood, that I do not pretend to have afcertained
the exact point in that fpace where the maximum falls:
the mean correfponds to the 62d degree, and yet I am
inclined to think that it is fomewhat lower, perhaps it
may be about the 57th: from this point, the condenfa-
tions of air downwards, and its expanfions upwards, fol-
low a diminifhing progreffion, compared with the con-
denfations and dilatations of quickfilver. The mano-
metrical are equal to the thermometrical fpaces, in two
points of the fcale; namely, at or near the freezing tem-
perature on one fide, and between the 112° and 132d
degrees of the fcale on the other. At the zero and boil-
ing point they are lefs than the thermometrical fpaces.
Whether this maximum of expanfion of air, compared
with that of quickfilver, be owing to moifture, or any

thing

thing elfe mixed with the former, which is brought into
its greateſt degree of action, about the temperature of
57° of FAHRENHEIT, muſt be left to the inveſtigations
of future experimenters: I only relate things as I found
them after many repetitions, without being able to dif-
cover any material difference in the reſults, even when
the air was rendered more or leſs denſe by an addition
to, or ſubtraction from, the weight wherewith it was
loaded. The thermometrical, compared with the mano-
metrical ſpaces, will therefore appear as in the following
table.

Spaces of the quickfilver thermometer, FAHRENHEIT's fcale.	Spaces of the manometer, meafured in degrees of FAHRENHEIT.
212°	212°
20	17.6
192	194.4
20	18.2
172	176.2
20	18.8
152	157.4
20	19.4
132	138.0
20	20.0
112	118.0
20	20.8
92	97.2
20	21.6
72	75.6
20	22.6
52	53.0
20	21.6
32	31.4
20	20.0
12	11.4
0	0

Experiments, for determining the actual expansion of common air, in the manometer, affected by the heat of 212°.

For this purpofe it became neceffary to afcertain, in every manometer, the exact proportion between the capacity of the tube and that of its bulb. This was done, by weighing the quickfilver that filled them refpectively, in a balance that was fenfible to a very fmall fraction of a grain. The contents of the bulb, and that part of the tube between it and zero, expreffed in grains, was called the air in experiment. The apparent expanfion of that air was meafured, by the grains that filled the feveral fections of the tube between zero and the boiling point; the fum being the total expanfion or increafe of volume, from a heat of 212°. The apparent expanfion, thus found, was again augmented for the dilatation of the tube, on the following principles.

In the firft part of this paper I have fhewn, that folid glafs rods dilate much lefs than barometer tubes. The mean between Mr. SMEATON's and my experiments, gives $\frac{14}{1000}$ of an inch for the longitudinal extenfion of every foot of thefe tubes, by 212°. From the rate of going of a clock, for near a year, whofe pendulum rod is folid glafs, its dilatation feems to be one-third part of a fteel rod,

or $\frac{58}{10000}$ on a foot, by 212°. Now, as the manometers refemble folid rods much more than they do barometer tubes, it is probable their dilatation, even allowing for the greater extenfion of the bulb, would not exceed $\frac{6}{1000}$ths of an inch on a foot, or $\frac{1}{1000}$th part on every two inches. In this ratio I have therefore augmented the apparent, to obtain the true, capacity of each manometer. The equation, amounting to about $\frac{1}{220}$th part of the whole, being lefs than the common error of fuch complicated obfervations, might in fact have been entirely omitted, without producing any material alteration in the refults.

Having, in this manner, computed the total increafed volume of any number of equal parts of air (according to the capacity of the bulb and tube in grains) and very often likewife the partial expanfions for intermediate temperatures, expreffed by the contents of the correfponding fections of the tube, I then found the ratio anfwering to 1000 equal parts, which, being divided by the degrees of difference of temperature, gave the mean rate for the whole fcale, or the particular rate for any intermediate fection of it.

The experiments, confidered in this way, are diftributed into four claffes, whereof the refults are comprehended in the four following tables. The firft fhews the expanfion of air, whofe denfity was much greater than that of the

common atmofphere. The fecond, which is divided into two fets, contains thofe on air that fuftained a preffure lefs than the atmofphere. In the third clafs, a very fhort column of quickfilver being employed to confine the air, its denfity differed little from that we commonly breathe in: this clafs is likewife fubdivided into two fets, and it will hereafter be made ufe of to regulate the equation depending on the temperature of the air, in the application of the barometer. The fourth and laft clafs of experiments, were made on air of the common denfity, artificially moiftened by the admiffion, fometimes of fteam, and at others of water, into the bulb; it is accordingly diftinguifhed into two fets.

TABLE I. Refults of experiments on the expanfion of air, whofe mean denfity was equal to $2\frac{1}{2}$ atmofpheres.

N°	Height of the barometer.	Inches of quickfilver confining the air.	Denfity in inches.	Total expanfion of 1000 equal parts of air by 212°.	Mean rate for each degree.
1	29.7	+72.	101.7	451.54	2.12991
2	29.7	+62.6	92.3	423.23	1.99637
3	29.62	+50.84	80.46	412.09	1.94382
4	29.66	+24.88	54.54	439.87	2.07486
5	29.66	+20.05	49.71	443.24	2.09075
		Mean,	75.74	434.00	2.04717

TABLE II. Refults of experiments on the expanfion of air of the denfity of five-fixths of the common atmofphere; and of others on air that was extremely rare, being only preffed with about one-fifth of an atmofphere.

N°		Height of the barom. quick-filver.	Denfity in inches.	Total expanfion of 1000 equal parts of air by 212°.	Mean rate for each degree.	Expanfion for intermediate temperatures.						
						From 0 to 32°.	32° to 52°.	52° to 72°.	72° to 92°.	92° to 132°.	132° to 172°.	172° to 212°.
1		29,85 — 5,44	24,41	495,455	2,33705		2,41666	2,64060	2,55200	2,46040	2,31850	2,20748
2		29,76 — 3,05	26,71	504,340	2,37896	2,27190	2,48150	2,63150	2,59650	2,15050	2,12000	2,10925
3	(First fet)	29,79 — 0,53	29,26	470,32	2,21849	1,99437	2,63150	2,59650	2,24800	2,54425	2,05325	1,83525
4		30,09 — 5,43	24,66	469,07	2,21259	2,32688	2,53450	2,66250	2,24800	2,54425	2,21375	1,83525
5		29,93 — 9,63	20,30	479,20	2,26038	2,14750	2,49500	2,59850	2,47900	2,25950	2,21375	2,11850
	Mean,		25,17	483,677	2,28140	2,16266	2,48191	2,63327	2,41087	2,28116	2,17637	2,06762

Not obferved. (From 0 to 32°.)

N°	Experiment	Difference of temperature.	Total expanfion.	Mean rate.
6 {	Experiment in a heated room in Philpot lane, February 25, barometer 30.°03 — 4.82 = 25.21 the denfity of the air,	on 113½ from 48½ to 162	244,604	2,15510
		62 — 100 to 162	118,293	1,90800
7 {	In Philpot lane; tube with a finall bore; barometer 30.03 — 23.2 = 6.77 the denfity. The air had been heated red-hot in the bulb before it was fealed,	on 113½ from 48½ to 162	138,75	1,22247
		51 — 48½ to 101½	71,93	1,41039
		60½ — 101½ to 162	66,82	1,10446
	The expanfion for 212°, at the mean rate, would be,	—	259,164	1,55890
8 {	In Pulteney ftreet, February 28th; with the fame manometer that had been ufed in the fame experiment in Philpot lane, barometer 30.08 — 23.2 = 6.88 the denfity of the air,	on 212 — 32 above zero.	330,487	1,55890
		20 from 32 to 52	44,574	1,39294
		20 — 52 to 72	37,771	1,88855
		80 — 52 to 132	139,784	1,74730
		60 — 132 to 192	94,804	1,58007
		20 — 192 to 212	13,554	0,67770
9 {	In Pulteney ftreet, April 19th; tube with a large bore, barometer 29.8 — 24.08 = 5.72 the denfity of the air, which had been heated red-hot in the bulb before it was fealed,	on 180 from 32 to 212	141,504	0,78613
		20 — 32 to 52	17,845	0,89225
		20 — 52 to 72	25,943	1,29715
		20 — 72 to 92	14,228	0,71140
		20 — 92 to 112	14,937	0,74685
		20 — 112 to 132	20,911	1,04550
		20 — 132 to 152	14,151	0,70755
		20 — 152 to 172	14,150	0,70750
		20 — 172 to 192	12,264	0,61320
		20 — 192 to 212	7,075	0,35375
	The Expanfion for 212° at the mean rate would be,	—	166,660	7,075
	Mean of the three means; denfity 6.46, expanfion for 212°,	—	252,104	1,18917

TABLE III. Refults of experiments on the expanfion of air of the denfity of the common atmofphere.

N°	Height of the barom.	Inches of quickfilver confining the air.	Denfity in inches.	Total expanfion of 1000 equal parts of air by 212°.	Mean rate for each degree.
1	29.95	+1.57	31.52	483.89	2.28250
2	30.07	+0.70	30.77	482.10	2.27406
3	29.48	+0.42	29.90	480.74	2.26764
4	29 90	+0.83	30.73	485.86	2.29182
5	29.96	+0.96	30.92	489.45	2.30870
6	29.90	+0.65	30 55	476 04	2.24547
7	29.95	+0 65	30.60	487.55	2.29976
8	30.07	+0 53	30.60	482 80	2.27736
9	29 48	+0 52	30.00	489.47	2.30871
Mean			30.62	484.21	2.28401

First fet; common air. { 1–7 }

Second fet; common air heated red-hot { 8, 9 }

The total expanfion 484.21 being divided into parts proportionable to the manometrical fpaces, meafured in degrees of the quickfilver thermometer, as already given; we have the following expanfions for intermediate temperatures, the rates for every 10° below 92° being found by interpolation.

Ther-

Thermo-metrical fpaces.	Manome-trical fpaces.	Total Expan-fions for degrees above zero, 1000 parts.	Difference of expan-fions, 1000 parts.	Rate for each degree, 1000 parts.
212°.	212°.	484.210		
			40.199	2.00995
192.	194.4	444.011		
			41.559	2.07795
172.	176.2	402.452		
			42.949	2.14745
152.	157.4	359.503		
			44.310	2.21550
132.	138.	315.193		
			45.680	2.28400
112.	118.	269.513		
			47.507	2.37535
92.	97.2	222.006		
			24.211	2.42110
82.	86.6	197.795		
			25.124	2.51240
72.	75.6	172.671		
			25.581	2.55810
62.	64.4	147.090		
			26.037	2.60370
52.	53.	121.053		
			25.124	2.51240
42.	42.	95.929		
			24.211	2.42110
32.	31.4	71.718		
			23.297	2.32970
22.	21.2	48.421		
			22.383	2.23830
12.	11.4	26.038		
			26.038	2.16983
0.	——	——		

Hence 222.006 – 26.038 = 195.968 = 2.4496, or 2.45, is the mean rate of expanfion for the 80° comprehended between 12° and 92° of FAHRENHEIT.

TABLE IV. Results of experiments on the expansion of air, artificially moistened, by the admission of steam, and sometimes water, into the bulb of the manometer.

	N°	Height of the barom.	Inches of quicksilver confining the air.	Density in inches.	Total expansion of 1000 equal parts of air by 212°.	Mean rate for each degree.	Expansion for intermediate temperatures.									
							from zero to 32°.	32° to 52°.	52° to 72°.	72° to 92°.	92° to 112°.	112° to 132°.	132° to 152°.	152° to 172°.	172° to 192°.	192° to 212°.
First set: steam admitted into the bulb before it was sealed.	1	30.16	+1.6	31.76	——	——	2.059375	2.60700	3.02650	3.38050	4.18300	6.48000	8.67750	11.93600	16.85050	——
	2	29.97	+2.2	32.17	1409.04	6.64642	2.20250	2.59250	2.90950	3.67650	5.16700	6.93300	10.17500	10.64200	16.57850	8.25400
	3	30.00	+2.2	32.20	1350.10	6.36840	2.26875	2.59100	3.04900	3.77550	4.36900	7.60500	8 94400	10.42950	11.92200	11.69000
	4	30.43	+1.92	32.35	———	——	2.20875	2.51450	2.74700	3.25500	3.73700	5.91350	9 18950	11.57550	25.88650	——
	5	30.2	+1.6	31.80	1999.71	9.43259	2.361875	2.51300	2.96400	3.84750	5.76100	7.19450	12.29850	16.69750	19.29500	25.23550
	6	30.32	+2.37	32.69	2576.16	12.15169	2.16250	2.55350	3.11600	3.72300	5.53600	7.83900	12.74100	16.74600	27.84350	45.25000
Second set: a drop of cold water admitted into the bulb before it was sealed.	7	30.2	+1.3	31.50	1135.48	5.35604	2.22594	2.74450	2.90500	3.47750	5.41900	6.16650	7.98850	8.58950	10.93600	4.98600
	8	30.06	+3.2	33.26	——	——	2.54062	2.63350	2.80850	3.78700	4.60750	——	Tube broken.		——	——
	9	30 32	+1.6	31.92	1538.31	7.25618	2.02156	2.54250	3.22500	3.76500	5 41700	6.79250	9.14350	9.71100	13.75550	19 93270
Mean,				32.18	1668.13	7.86854	2.22799	2.58800	2.97228	3.63194	4.91072	6.86550	9.89494	12.04087	17.88344	19.22470
Mean of the second, third, and seventh,				31.96	1298.20	6.12362	2.23239	2.64267	2.95450	3.64317	4.98500	6.90150	9.03583	9.88700	13.14550	8.31000
Mean of the fifth, sixth, and ninth,				32.14	2038.06	9.61349	2.18198	2.53633	3.10167	3.77850	5.57133	7.27533	11.39500	14.38483	20.29800	30.13940

By N° 1. the total expansion for 192° is 1208.72, whence the mean rate is 6.29542.

4.   — — 192° 1367.05, — — 7.12005.

8.   — — 112° 358.03, — — 3.19669.

From

From the experiments of the firſt claſs it appears, that 1000 equal parts of common air, loaded with two atmoſpheres and a half, being affected with a heat of 212°, expands 434 of thoſe parts; that is to ſay, in its dilated ſtate, it occupies a ſpace bearing, to that which it originally filled, the proportion of 1434 to 1000: hence the mean rate of expanſion of air of that extraordinary denſity is 2.04717 for each degree.

From the firſt ſet of the ſecond claſs of experiments it appears, that 1000 equal parts of air, preſſed only with $\frac{5}{6}$ths of an atmoſphere, and ſuffering a heat of 212°, expands nearly 484 of thoſe parts, whereof the mean rate for each degree is 2.28140. The maximum correſponds to that ſection of the ſcale between 52° and 72°; and the rate for the extremes is leſs than the mean.

But in the ſecond ſet of this claſs, when the confined air was rendered ſo extremely rare as to be preſſed with only one-fifth of an atmoſphere, in which caſe there was a neceſſity for heating it red-hot before it was poſſible to make the quickſilver hang in any tube of a moderate length, the expanſion of 1000 equal parts of air is, by the ſeventh and eighth experiments, diminiſhed to about two-thirds of the uſual quantity; and by the ninth, it is conſiderably leſs, amounting only to 141.5 for the 180°

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comprehended between freezing and boiling, or 0.78613 for each degree. The maximum ſtill correſponds to the ſpace between 52° and 72°; and the minimum is conſtantly at the boiling point.

From theſe three laſt experiments it would ſeem, that the particles of air may be ſo far removed from each other, by the diminution of preſſure, as to loſe a very great part of their elaſtic force; ſince, in the ninth experiment, the heat of boiling water applied for an hour together, could only make it occupy a ſpace which, compared with what it filled at freezing, bears the proportion of 1141.5 to 1000.

From the third claſs of experiments it appears, that common air, preſſed with a ſingle atmoſphere, whether taken into the manometer in its natural ſtate, or heated red-hot therein, has the ſame expanſion with air of only five-ſixths of that denſity: for 1000 equal parts of this air expanded 484.21 from 212° of heat, whereof the mean rate is 2.28401 for each degree. By comparing this reſult with that of the firſt claſs, and again with that deduced from the ſecond ſet of the ſecond claſs, it would ſeem, that the elaſtic force of common air is greater than when its denſity is conſiderably augmented or diminiſhed by an addition to, or ſubtraction from, the weight

6   with

with which it is loaded[f]; for, in the firft cafe, it bears the proportion of 484 to 434; and in the laft, it is (from the

(f) This difference between the elaftic force of common air, and that which is artificially rendered more or lefs denfe, by the addition or fubtraction of weight, particularly the latter, is truly remarkable, and contradicts the experience of BOYLE, MARRIOTTE, &c. It could not arife from the adhefion of the quickfilver to the tube, though in the denfe experiments a column of 72 inches was once made ufe of; becaufe the conftant motion given to the manometer before the fpaces were marked, muft either have prevented any irregularity whatever, or made the apparent expanfion fometimes too great, and at others too little. But the rare experiments ferve to put this matter out of doubt; for if the adhefion of the quickfilver to the tube had tended to leffen the apparent expanfion of the air, beneath the truth in one cafe, it muft have had a direct contrary effect in the other, and augmented it above the truth, which it evidently doth not.

Thefe experiments on the expanfion of air lefs denfe than the atmofphere, were extremely difficult and troublefome; and it was not till after feveral fruitlefs attempts that, with the affiftance of Dr. LIND, an apparatus was prepared for making them with fufficient accuracy. The veffel employed for this purpofe was made of the brafs tube of a large telefcope, near four inches in diameter; it was divided into four pieces, which, when fcrewed together, made a pot of fix feet in height. This was mounted on a platform laid over the area rails, for the reception of the manometer, which was placed therein, with the bulb uppermoft, the lower extremity of the tube paffing through a focket at the bottom of the veffel, and then through a collar compofed of many thickneffes of flannel. By means of a brafs plate and three long fcrews, the collar was made to embrace the tube fo clofely, as to fuffer very little water to pafs: fuch as did iffue, oozed off along the fides of a paper funnel, bound round the end of the tube, without entering into the bore. In this pofition, it required fome degree of force to pufh the manometer up, or draw it down, till the top of the quickfilver confining the air, juft appeared without the collar, fo as to admit the fpaces to be meafured, from a fixed point marked on the tube. The veffel being filled with boiling water, was kept to that temperature by means of lamps fuf-.

pended

the mean of three experiments) as 484 to 252, when preffed with only one-fifth of an atmofphere.

The

pended around it.   Two thermometers were made ufe of; the long one, whofe ball ftood at the bottom; and a fhort one at the top, that defcended no lower than juft to be immerfed in the water.   By fome of the firft of thefe experiments, the lamps not being placed directly at the bottom, water was perceived to be a very bad conductor of heat; for it would boil violently at the top, and the fhort thermometer there would mark 212°, while the long one would only mark 185° or 190° at the bottom.   By flow degrees the heat would neverthelefs defcend, and in the fpace of fifteen or twenty minutes the whole column would become of the fame uniform temperature.   But when the apparatus was adapted for experiments on air denfer than the atmofphere, in which cafe a plate of tin was foldered over the hole at the bottom, that it might be placed on a ftrong fire, the heat was then greateft below, and the long thermometer would mark upwards of 215°, while the fhort one ftood at 209° or 210°.   By defifting from blowing the fire, or removing a part of it, the particles of water fuffering the greateft heat would afcend, mix with the reft, and for fome little time make the whole column of an uniform temperature.   But the fire being totally removed, the top of the column in cooling was always hotteft; wherefore, in all thefe experiments, whether on denfe or rare air, great care was taken to mix the water thoroughly.

From Mr. DE LUC's book it appears, that M. AMONTONS found the effect of heat on the air confined in his thermometer, which feems to have been the fame fort of inftrument with the manometer, proportionable to the weight with which it was loaded.   By this he could not mean that, being of a double denfity, it had twice the expanfion.   I apprehend it muft here be underftood, that the fpaces the air occupied, were inverfely as the weights.   That being preffed with a double weight, it only filled half the fpace; or with half the weight, a double fpace.   This is no doubt nearly, though not accurately, the law that it follows. From thefe experiments it appears, that there is little difference in the actual expanfion or elaftic force of air, preffed with an atmofphere + or — one-third part: yet, when it is rendered extremely rare, its elafticity is wonderfully diminifhed. There feems likewife to be a vifible diminution in its expanfion, when loaded with two atmofpheres and a half.   Some of the tubes that I ufed were near nine

feet

The total expanfion 484.21 refulting from the third clafs of experiments, which are very confiftent among themfelves, being divided into parts proportionable to the manometrical fpaces, as meafured in degrees of the quickfilver thermometer, we have the expanfions for intermediate temperatures, exprefled at the bottom of the third table, where, it is to be obferved, the rates for every 10° below 92° were found by interpolation.

Now as barometrical obfervations will probably never be made in a temperature higher than 92° in the fhade, nor in one lower than 12°, if we fubtract 26.038, the expanfion anfwering to 12°, from 222.006, that which correfponds to 92°, we fhall have 195.968 for the 80 intermediate degrees; or 2.45 for the mean rate on each. This equation, compared with Mr. DE LUC's, bears the proportion of 245 to 210, which is a difference of $\frac{35}{100000}$ on every degree, or one-feventh part of the whole: and though this rate will be found hereafter to

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feet long. Had it been poffible to have managed them of double or triple that length, fo as to have admitted the air to be preffed with a column of 18 or 20 feet of quickfilver, I am perfuaded the diminution in the expanfion of air of that extraordinary denfity would have been much more perceptible.

Mr. AMONTONS found, that the condenfation of air in his thermometers kept pace with that of fpirit of wine, which we are told follows a decreafing progreffion with refpect to quickfilver: wherefore his experiments agree with thefe, in making the condenfation of air below 57° follow a decreafing progreffion, when compared with that of quickfilver.

exceed that deduced from the operations of the barome-
ter in extreme temperatures; yet they agree exceedingly
well with each other for the mean heat of the air, when
the barometer will come moſt frequently into uſe.

The fourth claſs of experiments are all that now re-
main to be mentioned. The bare inſpection of TABLE
IV. will ſhew, how greatly ſuperior the elaſtic force of
moiſt is to that of dry air. It is true indeed, that two
kinds of irregularities preſent themſelves among the re-
ſults: firſt, with regard to the total expanſion for $212°$;
and ſecondly, as to the greateſt exertion of the elaſtic
force, which ſometimes ſeems to have taken place before
the air has acquired the heat of boiling water. The
firſt is eaſily accounted for: it muſt have ariſen from dif-
ferent proportions of moiſture being admitted into the
ſame quantity of air, which there was no poſſibility of
aſcertaining, the bulbs and their apertures being of very
different dimenſions. With regard to the ſecond irregu-
larity, I am rather inclined to think that it may have
proceeded from error of obſervation, it being difficult to
determine the accurate temperature near boiling; eſpe-
cially when any part of the air roſe above the top of the
veſſel, which was ſometimes the caſe, notwithſtanding its
extraordinary height. Be that as it may, a very uniform
encreaſing progreſſion will be perceived to take place,

from

from the zero of FAHRENHEIT, as far as 152° or 172°;
and even to the boiling point, in thofe which I efteem the
beft experiments. By adhering to the mean refult it will
appear that air, however moift, having that moifture
condenfed or feparated from it by cold, its expanfion dif-
fers not fenfibly from that of dry air. Thus the rate for
32° below freezing 2.22799, is nearly the fame as in dry
air; but no fooner doth the moifture begin to diffolve
and mix with the air, by the addition of 20° of heat,
than the difference is perceptible: for inftead of 2.46675,
the rate for 20° above 32° in dry air, we have 2.588 for
that which is moift. In the next ftep of 20°, the rate for
dry air is 2.5809; whereas that for moift is 2.97. In
this manner the progreffion goes on continually encreaf-
ing, fo as to give 7.86854 for the mean rate on each de-
gree of the 212°, which is near three times and a half
the expanfion of dry air. And laftly, the rate for the
20° between 192° and 212°, is twice and one-half the
mean rate, and about nine times that which correfponds
to the zero of the fcale: but if the comparifon is drawn
from the mean of the fifth, fixth, and ninth experi-
ments, as being probably neareft the truth, the total ex-
panfion of moift, will be more than four times that
of dry air; and the rate for the temperature at boiling,
will

will be nearly fifteen times that which correſponds to the zero of FAHRENHEIT.

I am aware it will be alledged, that the proportion of moiſture admitted into the manometer in theſe experiments, is greater than what can ever take place in nature; and therefore, in order to be able to judge of the degrees of expanſion the medium ſuffers in its more or leſs denſe, and more or leſs moiſt ſtates, that not only air near the ſurface of the earth, but likewiſe that found at the top of ſome very high mountain, ſhould have been made uſe of. I grant all this: but on the other hand it muſt be remembered, that theſe experiments are very recently finiſhed; that a good hygrometer (if ſuch can ever be obtained) a great deal of leiſure time, and the vicinity of high mountains, were all neceſſary for the carrying of ſuch a ſcheme into execution.

It is for theſe reaſons, and in hopes that other people will, ſooner or later, inveſtigate this matter ſtill farther, not only by experiments made on the expanſion of air, taken at different heights above the level of the ſea in middle latitudes, but likewiſe on that appertaining to the humid and dry regions of the atmoſphere towards the equator and poles, that I have been induced to haſten the communication of this paper. In the mean time having proved, beyond the poſſibility of doubt, that a wonderful

2   difference

difference doth exift between the elaftic force of dry and moift air, I may be allowed hereafter to reafon by analogy, on the probable effects this will produce, in meafuring heights with the barometer; leaving it to others, much better qualified, to confider how far it will affect aftronomical refractions. In the following fection I fhall therefore give an account of the barometrical obfervations made in Britain, and compare them with fome others made in diftant countries.

S E C T I O N III.

An account of the barometrical obfervations made in Britain, wherein they are compared with fome others of the fame kind made in diftant countries.

THE revival of the inquiries into that curious and ufeful branch of philofophy, whereby vertical heights are determined to a great degree of exactnefs, by the preffure of the atmofphere alone, we owe to Mr. DE LUC; who hath undoubtedly removed many of the difficulties that formerly occurred in the application of the barometer, and thereby encouraged others to attempt to overcome, fome part at leaft, of fuch as remain.

If

If the rule deduced from the obfervations on Saleve had been abfolutely free from exceptions, and if there had not been particular points in the theory concerning which the ingenious author himfelf feems to have enter-tained doubts, it would probably have been univerfally adopted, without undergoing any very fcrupulous invef-tigation; but the obfervations made at Sun-rifing on Sa-leve, gave refults that were defective, or lefs than the real height. In certain cafes, the equation for high tem-peratures, remote from the zero of the fcale, appeared to follow a diminifhing, and in others an increafing pro-greffion. Hence arofe fome caufes of uncertainty, with refpect to the fpecific gravities of quickfilver and air, and the relative expanfion of one compared with the other; efpecially when the atmofphere happened to have more or lefs moifture diffolved in it. It was doubtlefs from confiderations of this fort, that Mr. DE LUC, in his book, fo ftrongly recommends the making of numerous fets of obfervations, in different countries; that, by the united labours of all, this interefting part of natural philofophy, might be brought ftill nearer to perfection.

With this general object in view, I am now to give an account of the principal barometrical obfervations that have been made in Britain, on heights determined geo-metrically with great care. Thefe heights are claffed in

the

the following lift in fix fets, according to the diftricts of the country wherein they are fituated, and nearly in the order of time in which the obfervations were made.

Height in feet.

		Height in feet
No I. Heights in and near London.	{ St. Paul's Church-yard *(g)*, North-fide, and iron gallery over the dome, — — —	} 281
	Top of Paul's-ftairs, and the faid gallery, —	324
	Top of Scotland-yard wharf, and the dining-room of the Spaniard on Hampftead-heath, —	} 422
	Great Pulteney-ftreet, and the faid dining-room, —	352
	Pagoda in Kew gardens, — — —	116½
	Gun wharf in Woolwich Warren, and uppermoft ftory of Shooter's-hill inn *(h)*, — — —	} 444

(g) Mr. BANKS, affifted by other gentlemen, meafured very accurately with a line the height of the ball of St. Paul's above the floor of the church, which was found to agree, exceedingly near, with that taken from the engraved fection of the building. The diftance of the ball from the dining-room of the Spaniard, was found by a bafe meafured on Hampftead-heath; and their relative heights by the angle of depreffion of the ball, taken with the aftronomical quadrant from the faid dining-room. The heights of Paul's-ftairs and Scotland-yard wharf, with refpect to each other, were found by meafuring from them feverally to the furface of high water in the Thames. And the relative heights of the church-yard and floor of the church with refpect to the ftairs, and of Pulteney-ftreet with regard to the wharf, were obtained by levelling to them refpectively.

(h) The height of Shooter's-hill inn, above Woolwich, was found by a bafe meafured in the meadows from the Warren eaftward. Lord MULGRAVE, Mr. BANKS, and Dr. SOLANDER, affifted in the geometrical operations; as did Dr. BLAGDEN, Meff. DE LUC and LLOYD, in the barometrical obfervations.

No.

Feet.

N° 2. near Tay- bridge in Perthſhire.	Station at the Eaſt-gate of Caſtle Menzie's gardens near the village of Weem, and top of Weem Craig,	700¼
	The ſaid ſtation, and top of Bolfracks Cairn, —	1076½
	The ſaid ſtation, and top of Dull Craig, —	1244½
	The ſaid ſtation, and top of Knock Farle, —	1364½
	The ſaid ſtation, and that at the rivulet of Glenmore, below the South obſervatory on Schihallien, —	1279¼
	The ſaid ſtation, and South obſervatory, — —	2098
	The ſaid ſtation, and Weſtern ſummit of Schihallien,	3281
	Station at the rivulet of Glenmore, and the South ob- ſervatory, — — — —	818¾
N° 3. near Lanark.	Level of the Clyde at Lanark-bridge, and ſtation in the garden at Lanark, — —	362½
	Ditto level, and top of Stonebyre-hill, — —	654
	Robinhood's well, before Carmichael-houſe, and top of Tinto, four feet below the ſummit of the Cairn,	1642½
	Ditto well, and Weſt end of Carmichael-hill, —	451½
N° 4. near Edin- burgh.	Leith pier-head, and top of the Calton-hill, —	344
	Leith pier, and ſummit of Arthur's Seat, — —	803
	Leith pier, and Kirk-yetton Cairn, on the Eaſt-end of the Pentland hills, — — —	1544
	Calton hill, and ditto Cairn, — — —	1200
	Level of Hawk-hill ſtudy, and top of Arthur Seat,	702½
	Hawk-hill obſervatory, and bottom of the little rock on Arthur Seat, 7¼ feet below the ſummit, —	68.¼
	Hawk-hill garden-door, and ditto little rock, —	730¼
N° 5. near Lin- houſe.	Linhouſe, and Eaſt Cairn-hill, 5 feet below the ſummit,	1176½
	Ditto, 18 feet below the top, — —	1165¼
	Linhouſe, and Weſt Cairn-hill, 11 feet below the top,	1178½
	Ditto, and Corſtown-hill, 4 feet below the top, —	386½
	Corſtown-hill, and Weſt Cairn-hill, — —	792
	Ditto, and Eaſt Cairn-hill, — — —	776½
N° 6. near Carnar- von in North Wales.	Carnarvon Quay, and Snowdon Peak, — —	3555
	Ditto, and ſummit of Moel Eilio, — —	2371

To enter into a minute detail of the geometrical ope-
rations, whereby the whole of theſe vertical heights

were

were determined, would be extremely tedious and unin-
terefting. That fome idea may however be formed of
the degree of accuracy with which they were afcertained,
it will be fufficient to obferve, that the requifite angles
were taken with an aftronomical quadrant of a foot ra-
dius, made by Mr. sisson, and curioufly adapted for the
meafurement of horizontal or bafe angles; which, as well
as thofe of the vertical kind, might always be determined
thereby to within ten feconds of the truth. The bafes
were meafured with care; and, in order to afcertain the
diftances, the three angles of each triangle were, as
often as poffible, actually obferved with the quadrant.
That the variation of the line of collimation of the in-
ftrument, which was found to alter in carrying, might
occafion no error, one or more of the angles of elevation,
at each ftation, were taken on the arc of excefs, as well as
on the quadrantal arc. In all cafes, the ufual [i] allow-
ances were made for curvature and refraction: and for
the correction of the laft, fometimes the angles of de-

(i) If the fquare of the diftance be divided by the diameter of the earth, the
quotient will give the curvature of the globe on that diftance, or the excefs of
the apparent above the true level: and, by Mr. maskelyne's rule, the fquare of
the diftance being divided by the diameter of the earth, augmented by one-
fourth part, we have the allowance for curvature and refraction; which laft is
fuppofed to raife the object, by an angle equal to that of a great circle fubtended
by one-tenth part of the diftance.

preffion

preffion as well as of elevation were taken. When time would permit, the geometrical operations were repeated at the firft ftations; or the angles of elevation were obferved from fome new point connected with the firft, and whofe relative height, with refpect to the others, was known. Small altitudes were occafionally determined by levelling from one ftation to the other.

To prove that the vertical heights, affigned to the places in the preceding lift, are exceedingly near the truth, I need only mention the following inftances, by way of confirmation. In 1771, with the affiftance of Dr. LIND and his friend Mr. HOY, I meafured a bafe from the obfervatory of Hawk-hill weftward, whereby the height of the fummit of Arthur's Seat, above the telefcope of the Hawk-hill quadrant, in its horizontal pofition, was found to be 685.66 feet. In 1775, thefe gentlemen levelled, three feveral times, from the fummit downwards to the faid telefcope; and found the vertical diftance to be, by the firft operation, 686.47 feet; by the fecond, 684.43; and by the third, 685.25. This laft, which, from the great care that was ufed, they confidered as the beft, differs only three-tenths of a foot from the geometrical refult. They afterwards continued the operation of levelling from Hawk-hill to the pier of Leith, and having repeated it twice, with a difference of only two inches

between

between the refults, they found the mean defcent to be 117.38 feet: hence Arthur's Seat is above Leith pier, by the mode of levelling, 802.66; and by the mixed method 803 feet.

In 1774, when the aftronomer-royal was carrying on the Society's experiments for afcertaining the attraction of Schihallien, I found, from my own geometrical operations, depending on a bafe meafured in the plain near Taybridge, the Weftern fummit of the mountain to be 1183 feet above the South obfervatory.

Of this height, the effect of curvature and refraction amounted to 28.86 feet, on the diftance of Bolfracks Cairn from the obfervatory; and to 38 feet, on the diftance of the faid Cairn from the fummit of Schihallien. The refult of thefe operations I communicated to Mr. MASKELYNE, before his trigonometrical operations were begun. From the data which he hath fince been fo obliging as to furnifh me with, depending on the bafe in Glenmore at the bottom of Schihallien, and the angles of elevation taken from the Southern extremity of that bafe, the Weftern fummit of the mountain is 1186.6 feet above the South obfervatory. But if the triangle that ferved to connect the ftation of the barometer in that valley with the others, and the angles of elevation taken from the fame ftation are made ufe of, the dif-

ference

ference of height will be 1183.33 feet. Laftly, Mr.
MASKELYNE's refult, from the triangles on the North-fide
of the mountain, makes it 1180. The mean of thefe
three 1183.31 feet, is the height of the Weftern fummit
of the mountain above the South obfervatory, which only
exceeds my height by one-third of a foot. Here it is to
be obferved, that from the vicinity of thefe triangles to
the mountain, and the fhortnefs of the fides, the greateft
curvature amounted only to 16 or 17 inches, which con-
fequently made the effect of refraction next to nothing.
This near agreement between the refults feems there-
fore to prove, that the mode of computation for curva-
ture and refraction, made ufe of in the Taybridge obfer-
vations, is juft.

By the firft angles of elevation, taken from the ftation
of the barometer in Glenmore, the Weftern fummit of
Schihallien is 2001.88 feet above it; from which, if we
deduct 1183.33, there remains 815.55 for the height of
the South obfervatory above the faid ftation: but if the
laft angles of elevation at the ftation of the barometer
are made ufe of, the height between it and the obferva-
tory comes out 818.97, whereof the mean is 818.76
feet. Though thefe inftances are of themfelves fuffi-
cient to prove, that the geometrical heights may be fafely
depended upon; yet, as an example of the method that

was always made ufe of, I fhall annex to this paper, a plan of the triangles and detail of the operations for obtaining the height of Snowdon; becaufe that mountain, at the fame time that it is the higheft I have meafured, is, from its fituation, more likely to be vifited, and to have experiments repeated upon it, than the remote hills of the North.   I now proceed to give fome account of the barometrical obfervations.

The heights in and near London being fo very inconfiderable, it was eafily forefeen, that nothing conclufive could be drawn from obfervations made on them alone. It was, however, natural enough to try, even on thefe, whether the rule we had been furnifhed with would anfwer? A fmall height of 41 feet 4 inches, which, without inconveniency, could be recurred to at all times of the day, and all feafons of the year, was the firft that was made ufe of.   St. Paul's, Hampftead, Kew pagoda, and Shooter's-hill, were the next. The mean refults of many obfervations on the three firft, and of feveral on Shooter's-hill, were found to be defective. In general the coldeft obfervations, made in the morning and evening, when the temperatures at the two ftations differed leaft from each other, anfwered beft. In the hotteft part of the day, when that difference was the greateft, the refults were moft defective.

Some

Some months fpent in Scotland in the fummer of 1774, afforded opportunities of making barometrical ob‐fervations on hills of various heights, from three or four hundred, to upwards of three thoufand feet, as hath been exhibited in the preceding lift. That feafon was re‐markably cold and wet; wherefore, in thefe obfervations, the mean temperature of the air in the fhade was com‐monly about 55°. The hotteft never exceeded 63° in the plain; and the coldeft, namely thofe on the higheft mountains, were generally from 43° to 48°.

From the defect found in the refults of thefe obferva‐tions, which, with refpect to temperature, correfpond to the mean and hotteft of thofe made at Sun-rifing on Sa‐leve, and without any exception whatever, I could eafily difcover, either that a much greater equation than what the rule directed, muft be applied for each degree of heat above the zero of the fcale; or, that the zero itfelf would fall confiderably lower than 39°74, where Mr. DE LUC's formula, adapted to Englifh meafures, hath fixed it. This firft ftep towards a correction of the rule, naturally pointed out the fecond thing to be aimed at, namely, the obtaining of a fufficient number of cold obfervations, near the zero, and as far as poffible below it, that the equation might difappear entirely, and even come to be applied with the contrary fign. Of this kind the winter

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feafons of 1774 and 1775 afforded a few on the fmall heights in and near the metropolis; but the beft I have been furnifhed with are thofe which Dr. LIND, affifted by Mr. HOY, was fo obliging as to make on Arthur's Seat near Edinburgh; and thofe which Captain CALDERWOOD has fince favoured me with on the Cairn-hills, being a part of the Pentland range to the South-weft of that city.

By comparing thefe fets of obfervations together, it appeared from all of them, that when the air was at or near the freezing temperature, the logarithmic differences gave the real height, in Englifh fathoms and thoufandth parts, without any equation; and when confiderably below that point, the equation was to be fubtracted, or applied with the fign − inftead of +. It was farther perceived, that the fame general conclufion might be drawn from the coldeft, not only of the Sun-rifing, but even of the ordinary obfervations on Saleve, fome reduction of the temperature being in certain cafes made, on account of the expofure of the thermometer to the Sun's rays: hence I was led to fuppofe, that the morning obfervations, inftead of being made exceptions from the rule, were thofe, which, it might be prefumed, would form the beft bafis for deducing the equation depending on the heat of the air; becaufe the mean temperature of the column was then found to differ leaft from that of its.

extre-

extremities; whereas in the hotteft time of the day, that difference was generally the greateft.

Having been enabled, by means of the cold obfervations, to form fome judgement whereabout the zero of the fcale would fall, below which the equation was negative, and above it affirmative; it followed of courfe, that the next principal thing to be fought for, was the maximum of equation, or that correfponding to the higheft temperatures the climate of our ifland would afford. It was partly with the view of obtaining thefe that I went, in July 1775, to Snowdon in North Wales. On this expedition Captain CALDERWOOD was fo obliging as to accompany me, and lend me his affiftance in the operations for determining the geometrical height of that remarkable mountain. At that particular period, the weather proved unfavourable for obtaining hot barometrical obfervations; but, in other refpects, they were very fatisfactory, as being in general confiftent among themfelves, and agreeing fufficiently near with thofe of the preceding year in Scotland; at the fame time that they were made on a height, as formerly mentioned, greater than any other hitherto meafured, with equal care, in Britain. During the fummer of 1776, Dr. LIND obtained fome more hot obfervations on Arthur's Seat; and in the beginning of the following winter, Captain CALDERWOOD

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made others of the cold kind, on the Cairn-hills in his neighbourhood. From the combination of the whole of thefe obfervations taken together, and a comparifon of them with Mr. DE LUC's, as far as they are fimilar, I mean to fhew the agreement or otherwife, between the equation for the heat of the air, as deduced from the barometer and manometer; but fince the Britifh obfervations, in certain cafes, differ confiderably in their circumftances from thofe on Saleve, it is neceffary, in the firft place, to point out wherein this difference chiefly confifts.

In the obfervations in Britain, the barometers and detached thermometers have been, almoft - conftantly, placed in the open air in the fhade, and fuffered to remain there generally half an hour, and fometimes a great deal longer, before the correfponding obfervations were begun, that the quickfilver might have time to take the temperatures of the fituations refpectively. They were then obferved four times, ufually at intervals of ten minutes, the mean of the four being that which is calculated, and called a fingle obfervation. If the time did not admit of fo long an interval, the fame number of obfervations were taken at diftances of five minutes from each other. In either cafe, the extremes never differed above a few thoufandth parts of an inch from the mean, fo as to

render

render the computations of them feparately wholly un-neceffary.

Except in very fmall heights, and chiefly in London, where it was impoffible to fcreen the upper barometer fo effectually from the Sun during the time of obfervation as that below, which generally ftood in the fhade of fome building, the temperature of the quickfilver in the fu-perior [k] hath been colder than that in the inferior baro-meter. The difference was commonly found to be two or three degrees; fometimes it would amount to fix or feven; rarely, in heights that were confiderable, to nine or ten; and in one inftance only to thirteen, where the vertical diftance of the inftruments was great.

Whether in the plain or on the tops of the higheft mountains, the detached thermometers, indicating the temperature of the air, have generally ftood fomething

(k) I have fometimes found, particularly in frofty weather, that a thermo-meter placed on the pavement of the North-fide of St. Paul's Church-yard, clofe to the wall of the building, would ftand two degrees lower than that which was expofed on the North-fide of the iron gallery over the dome. The firft, no doubt, felt the cold produced by the evaporation from the ftones, while that above might be affected by the afcending fmoke. But the moft remarkable inftance of this kind occurs in one of Dr. LIND's obfervations, on the breaking up of the hard froft January 31, 1776: at Hawk-hill, at 10^h 45$''$ A.M. the temperature of the open air was 14°, while that at the fummit of Arthur's Seat was 20°. The froft that remained in the ground kept the air extremely cold below, though it had already felt the effects of the thaw at the top of the mountain.

lower

lower than thofe attached to their refpective barometers, until they had remained a confiderable time in the fame fituation, equally fhaded from the Sun, when they always agreed: whence it followed, that in thefe obfervations, the mean temperature of the air, and equation depending upon it, might always have been determined very near the truth, from the temperature of the quickfilver in the tubes, as fhewn by the attached thermometers, without ever confulting the detached ones. Let us now fee what were the circumftances attending the obfervations on Saleve.

Mr. DE LUC's lowermoft barometer ftood in the ground-ftory of a houfe near Geneva, where it remained unaltered during the whole of his experiments; while the detached thermometer, indicating the temperature of the air, was expofed on a fmall eminence, at a little diftance, directly to the Sun's rays: hence we find that, in the obfervations of high temperatures, the bottom of the column of air is often 12° or 15°, and in one cafe 18°, hotter than the quickfilver in the tube. And even in the loweft temperatures, the bottom of the column of air is generally hotter than the quickfilver within doors, contrary to common experience in this country: for in England, in winter, the exterior air in the fhade is always colder than the interior air. This circumftance gives

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reafon to apprehend, that the thermometer fuffered not only direct but reflected heat.

The fuperior barometer was fhaded with a parafol from the Sun, while its correfponding detached thermometer was expofed to his rays: wherefore, in the obfervations of high temperatures, the top of the column of air is ufually four or five degrees hotter than the quickfilver in the barometer ftanding in the fame air; and the mean heat of the column often exceeds very confiderably the mean heat of the quickfilver in the tubes.

In many of the coldeft of Mr. DE LUC's obfervations, as well as in thofe of mean temperatures of about 50° or 55°, the fuperior barometer is often the hotteft of the two, even when the furrounding atmofphere at the top is colder than at the bottom. This circumftance is eafily accounted for: wood is known to be a bad conductor of heat, to receive it flowly, and retain it long: that barometer, which was moved about from place to place upon the mountain, with a very fhort interval between the obfervations (as is fufficiently evident from the great number of ftations it paffed through in a limited time) muft have acquired and retained heat fuperior to that of the atmofphere, and communicated it to the tube with which it was in contact. Some difference would no doubt arife from this caufe, if the temperatures of the

<div align="right">quickfilver</div>

quickfilver in the tube and attached thermometer did not keep exactly pace with each other.

The laft point to be mentioned is ftill more remarkable than the reft; it is briefly this: in the obfervations at Sun-rifing on Saleve, though the fuperior quickfilver is the coldeft; yet the top of the column of air is commonly five or fix, and fometimes eight or nine degrees, warmer than the bottom.

Having thus fhewn the fteps that were taken, for obtaining the coldeft and hotteft barometrical obfervations that the climate of this ifland would afford, the mode of obferving, and wherein the circumftances attending them differed from thofe on Saleve, I fhall now point out the general refult. In order to avoid repetitions as much as poffible, it is neceffary, once for all, to remark, that the computations of the Britifh obfervations, by the rule hereafter to be given, are fubdivided into their refpective claffes. Each table contains 15 columns, which their titles fufficiently explain, that the principles from which the rule was deduced, the refult and error, might all appear in one view. The laft column towards the right-hand fhews the ratio of the weight of quickfilver to air, the columns of the firft in the barometers being feverally reduced to the mean temperature of the laft.

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By comparing the tables it will be found, that the ob-
fervations for extreme temperatures belong to the Edin-
burgh clafs of obfervations (N° 4.) it being thought beft,
in this cafe, to omit the few hot ones obtained on the in-
confiderable heights near London: the mean of the
coldeft, anfwering to the temperature of $21°.75$, make
the logarithmic excefs $\frac{29}{1000}$; and the mean of the hotteft,
correfponding to the temperature of $69°.6$, give a defect
of $\frac{81}{1000}$. Now the fum of the two equations $\frac{110}{1000}$, being di-
vided by the difference of temperature $47°.85$, we have
nearly 2.3 for the mean rate of the equation on each
degree, which is lefs than that refulting from the opera-
tions of the manometer. Again, from the mean of the
very beft obfervations, as being made on the greateft
heights, when the temperature of the air is $52°$, it ap-
pears, that the defect is from $\frac{49 \text{ to } 50}{1000}$, or 2.5 for each
degree nearly, which agrees perfectly well with the ma-
nometrical expanfion. In this cafe, the ratio of the
weight of quickfilver to air is as 11377 to 1; greater very
confiderably than 11232 to 1, affigned to them by Mr.
DE LUC, when the temperature is $69°.32$, anfwering to
the zero of his fcale, without any allowance for the di-
minution of preffure on his columns, which fhould have
rendered air ftill comparatively lighter. From the Bri-
tifh obfervations, made on the moft confiderable heights,

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it appears, that when the temperature of the air is 28°.2, the ratio of its weight, with refpect to that of quickfilver, is as 1 to 10552: hence the increafe of the weight of air, on every degree of difference of temperature between 28°.2 and 52°.5, amounts to 34.4; and hence we have 52°.5−4°.2=48°.3 for the temperature of the air in Britain, when its weight would be $\frac{1}{11232}$ of that of quickfilver; and confequently agree with Mr. DE LUC's, though the heat would differ from his 21°. It will no doubt be remarked, that the equation for the air, refulting from the operations of the barometer, falls fhort of that given by the manometer. Part of the difference, I apprehend, may arife from the fmall number of barometrical obfervations obtained in extreme temperatures. I fhall, neverthelefs, adduce reafons hereafter for fuppofing, that it really fhould diminifh, becaufe of the drier and lefs elaftic ftate of the fuperior air, compared with that taken into the manometer at the earth's furface. In the mean time, fince both inftruments agree in the equation for 52°, which is a heat that the barometer will very frequently be ufed in, it feems beft to adhere to the mean manometrical refult 2.45, in fixing the zero of the fcale, which is obtained in the following manner.

Divide the excefs or defect, expreffed in 1000th parts of the logarithmic refult, by 2.45, the mean expanfion of

air

air for each degree of the thermometer; the quotient will give the number of degrees, in the firſt caſe, to be added to, and in the laſt ſubtracted from, the temperature of the air in the obſervation; the ſum or difference anſwers to the zero of the ſcale, or that temperature when the logarithmic reſult gives the real height in Engliſh fathoms and 1000th parts.

According to this mode of computation, we have, from the aggregate of the ſeveral claſſes of Britiſh obſervations, the place of the zero as follows:

			Zero.
By the 1ſt claſs of obſervations in and near London, between the temperatures of			25.5 and 71.2 at 32.2
2d, near Taybridge,	—	—	46.1 — 62.9 – 31.1
3d, near Lanark,	—	—	44. — 62. – 32.8
4th, near Edinburgh,	—	—	17. — 70.7 – 31.5
5th, near Linhouſe,	—	—	26.1 — 46.5 – 29.0
6th, near Carnarvon,	—	—	40.1 — 62.3 – 32.9
Mean place of the zero at	—	—	31.7

The number 31°.7 differing ſo very little from 32°, we may hereafter conſider that remarkable point of FAHRENHEIT's thermometer, as the zero of the ſcale depending on the temperature of the air; and hence is deduced the ſecond part, of the rule for meaſuring heights with the barometer. When the mean temperature of the column of air to be meaſured is at 32° of FAHRENHEIT, the

difference

difference of the common logarithms of the equated heights of quickfilver in the inferior and fuperior baro-meters, expreffed in 1000th parts of an inch, gives the real height in fathoms and 1000th parts, the three figures towards the right-hand being decimals, and the reft in-tegers; which, being multiplied by fix, gives the refult in feet.

Let us next confider, in a general way, how far this will correfpond, or otherwife, with Mr. DE LUC's obfer-vations in extreme temperatures.

I have already had occafion to remark, that when the temperature of the air was at 69°.32, as indicated by thermometers expofed to the Sun's rays, Mr. DE LUC found that the differences of the common logarithms of the heights of the barometers at the two ftations, gave the altitude between them, in French toifes and 1000th parts: in which cafe the fpecific gravity of quickfilver to air was as 11232 to 1. When his formula is adapted to Englifh meafures, the zero of the fcale neceffarily de-fcends to 39.74, where the Englifh fathom bears the fame proportion to the modulus of the common loga-rithms, as, in the former cafe, the French toife did to that modulus, the equation for the intermediate temperature being now applied with the contrary fign. As it hath been fhewn, that the Britifh obfervations differ in their

circumftances from thofe on Saleve, and require a greater equation, it is unneceffary to enter into any minute comparifon of the two fets : neverthelefs, that fome idea may be formed of the caufe, of part at leaft, of the difference that takes place between them, I have collected into one view, the computations of fuch as were made in extreme temperatures; namely, the coldeft of thofe at Sun-rifing (though the whole of that clafs were confidered as exceptions from the rule); the coldeft and hotteft of the ordinary obfervations; alfo thofe on the Dole[1], at Genoa, and at Turin, whereby the heights of the lake of Geneva and of Turin, above the fea at Genoa, were obtained. In the table it will be obferved, that there is a column for the reduced temperature of the air, on account of the expofition of the thermometer to the Sun's

(1) Having recomputed the whole of Mr. DE LUC's obfervations on Saleve, and claffed them according to the months in which they were made, I intended, at one time, to have given a general table, comprehending the mean refults of all of them: however, this is now become unneceffary, fince a very refpectable and ingenious member of this Society hath had opportunities of making many curious and interefting obfervations on thofe very heights, which cannot fail of being perfectly fatisfactory; and who, at my requeft, was fo obliging as to determine the height of the Dole geometrically. On this mountain Mr. DE LUC had made barometrical obfervations, whofe refults differing confiderably from the altitude, 4182 feet above the lake of Geneva, as taken by Mr. FATIO DE DUILLIER, made me fufpect there was an error. In fact it appears, from the laft meafurement, that the fummit of the Dole is 4293 feet above the furface of the lake, which gives for the vertical diftance of Mr. DE LUC's barometer 4210 feet.

rays :

rays: I apprehend that I have not exceeded, but rather fallen fhort, in the reduction, to what would have been indicated by thermometers in the fhade, perfectly free from direct and reflected heat, and with fufficient time allowed between the obfervations. Be this as it may, it is of no importance, as no other conclufion is drawn from thefe obfervations, than that of fhewing what, in my apprehenfion, might probably have been the cafe, if another mode had been adopted.

From the table it appears, that when the temperature of the air is at 29°.5, the logarithmic excefs is $\frac{9}{1000}$; and at 75°.5 reduced temperature, the defect is $\frac{96}{1000}$. The fum of the two equations $\frac{105}{1000}$ being divided by the difference of temperature 46°, we have, as in the Britifh obfervations, nearly 2.3 for each degree, which is greater than that applied by Mr. DE LUC's rule, in the proportion of 23 to 21. That too fmall an equation hath been made ufe of in thefe hotteft obfervations, fuppofing the original zero and temperature to remain, is fufficiently evident: for $\frac{96}{1000}$ being divided by 42° the difference of temperature, we have, as before, 2.3 very nearly for the equation anfwering to each degree.

Farther, if we confider the ratio of the weight of quickfilver to air, actually refulting from the obfervations themfelves, the fame kind of error (for I cannot fee

it

it in any other light) ftill exifts. Thus, in the coldeft of the morning, as well as in the ordinary obfervations, when the temperature is at or near freezing, the mean ratio of the weight of quickfilver to air, is about 10850 to 1. When the obferved and reduced temperatures are refpectively 41° and 35°, the ratio between them is that of 11.295 to 1, anfwering nearly to what hath been affigned to them when the heat is 69°.32. Again, in the hotteft obfervations of the 14th and 15th of July 1759, and 20th of July 1760, on the higheft, and confequently the beft ftations, when the obferved and reduced temperatures are refpectively 81°.7 and 75°.6, quickfilver is to air as 12650 to 1. Now if we reduce this number 12650 by a proportionable part, for the degrees of difference between Mr. DE LUC's zero 69°.32, and the obferved and reduced temperatures refpectively, we fhall have, in the firft cafe, 12200; and in the laft, 12390 to 1, for the ratio of the weight of quickfilver to air; either of which exceeds very confiderably 11232, which hath been affigned to them.

With regard to the obfervations on the Dole, the defect is $\frac{81.4}{1000}$, anfwering to the obferved temperature of 66°.6, and which is only reduced to 65°.2. On this great height, the ratio of the weight of quickfilver to

air$^{(m)}$ is that of 12595 to 1. Mr. DE LA CAILLE's obfer-
vations at the Cape of Good Hope, annexed to the table
containing Mr. DE LUC's, give a defect of $\frac{58.7}{1000}$, when the
temperature feems to have been about 58°, in which cafe
quickfilver was 11687 times heavier than air.

Now if, from the aggregate of thefe obfervations, the
fame method be adopted, as was made ufe of in the Bri-
tifh, for finding the zero of the fcale, we fhall have it as
follows:

By Mr. DE LUC's equation for the air and obferved temperature.			By the manometrical equation and reduced temperature.		
	°	°	°	°	°
Coldeft of the morning obfervations, from	25.2 to 30.5	at 33.7	from 25.2 to 30.5	at 33.12
Coldeft of the ordinary obfervations,	27.1 — 41.9	— 38.7	—— 26. — 35.	— 32.97
Hotteft of the ordinary obfervations,	76. — 84.5	— 36.2	—— 73.5 — 77.	— 36.32
On the Dole, —	59.2 — 71.5	— 27.6	—— 58. — 70.	— 32.
Light houfe of Genoa,	75. — 79.	— 26.	—— 75. — 79.	— 33.40
DE LA CAILLE's, —	58.	— 30.	— — 58.	— 33.35
Zero at — —		32.03	Zero at	33.52

(m). It will even be found, though the calculations are not inferted in the
table, that the hotteft of Mr. DE LUC's morning obfervations, June 8th, 1758, at
the 15th ftation, anfwering to the mean temperature of 57°.5, and which I
confider as the beft, becaufe no reduction is neceffary for the expofition of the
thermometer, agree with the manometrical experiments, in requiring a greater
equation than is wanted in extreme temperatures: for in this cafe, the
mean of two obfervations gives a defect of $\frac{65.5}{1000}$ for 25°.5 above freezing, which
is 2.57 for each degree; the ratio of the weight of quickfilver to air being that
of 12196 to 1.

From

From the mean of thefe obfervations, though the re-
fults are irregular among themfelves, it appears fuffi-
ciently evident, that if the morning obfervations on Sa-
leve had been retained, inftead of being made exceptions
from the rule, the zero of the fcale would have de-
fcended about 8°; *viz.* from 69°.3 to 61°.4 of FAHREN-
HEIT, fuppofing always the equation 2.1 for each degree
of temperature, and the French toife, as the ftandard
meafure, to have been adhered to: for the French toife
bears to the Englifh fathom, the proportion of 106575
to 100000; wherefore $\frac{6575}{106575}=\frac{61.69}{2.1}=29°.4+32°=69°.4$,
denotes the relative pofitions of the two zeros, the inter-
mediate equation $\frac{61.69}{1000}$ being to be fubtracted, when the
toife is made ufe of. But it hath been fhewn, that the
mean expanfion of air is really greater, for fuch tempe-
ratures at leaft as the barometer can be applied in, than
what Mr. DE LUC fuppofed it, in the proportion of 245 to
210; whence it follows, that $\frac{61.69}{2.45}=25°.18+32°=57°.18$,
will denote the relative pofitions of the two zeros; which,
inftead of almoft 30°, are only diftant from each other a
little more than 25°.

From what hath been faid it is eafy to fee, that in cal-
culating heights according to Mr. DE LUC's rule, when
the temperature of the air is below his zero, which we

<div align="right">may</div>

may take at 40°, the Englifh meafure being ufed, the common error in the refult will be equal to the fum of the two equations, $2.1 + 2.45 = 4.55$ for each degree; which amounts to $\frac{36.4}{1000}$ parts for the 8° that the zero is too high. Above 40°, the former error $\frac{36.4}{1000}$ will be augmented by the difference of the equations for each degree that the temperature is above his zero, *viz.* $2.45 - 2.1 = \frac{0.35}{1000}$. In either cafe it is to be obferved, that the progreffive rate of equation for the heat of the quickfilver is not here taken into the account; becaufe it will not produce any material difference, unlefs one barometer is much hotter than the other, at the fame time that their vertical diftance is very great. Thus the 32d degree of FAHRENHEIT, or freezing temperature, which is fundamental in all thermometers, happens, fomewhat remarkably, to be the zero of the fcale, when the Englifh fathom bears fuch proportion to the modulus of the common logarithms, as that their difference, in computing heights by the barometer, brings out the refult in fathoms. No other proportion of a meafure will do it: for if we fuppofe twenty-four of different lengths, between ours and the French toife, each furpaffing the other by $\frac{263}{100000}$ of that toife, the zero of the fcale, in computing heights by thefe meafures refpectively, will afcend a fingle degree.

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gree on each; and the French toife being the 25th, will have its zero nearly at the 57th degree: about which temperature the expanfion of air appears, from the experiments, to be at its maximum. From that point, therefore, the equation will diminifh both ways, though by a quicker progreffion for condenfation, than it doth for dilatation.

Having thus compared, in a general way, the refults of the Britifh obfervations with thofe of Mr. DE LUC, pointed out what feem to be the chief caufes of the conftant defect found in his rule, and thereby obtained, it is hoped, fome corrections tending to improve the theory of the barometer, when applied to the meafurement of heights in middle latitudes; it remains to fhew the principles, whereon the table for the equation of the air hath been conftructed. Previoufly however to this, it may be proper to compare, with as much brevity as poffible, thefe obfervations, with others that have been made towards the Pole and at the Equator: from which it will appear probable, that the rule which anfwers in middle latitudes, will not in the frigid and torrid zones.

In 1773, Captain PHIPPS, now Lord MULGRAVE, commanding two of his Majefty's fhips then fent on difcoveries towards the North Pole, meafured geometrically, with great care, the height of a mountain in Hakluyt's Ifland

near

near Spitzbergen, and found it to be 1503 feet above the level of the sea. On the morning of the 18th of August, the following observations, at the sea-shore and top of the mountain, were made with a single barometer, wherein the quicksilver had not been boiled.

At 6 h. A.M. Barometer at the shore,   —   —   30.040   therm. 50°
  7 h. 45′ A.M. Ditto at the top of the mountain,   28.266   ——   42
  8 h. 45′ A.M. Ditto at the top of ditto,   —   28.258   ——   42
  11 h. 45′ A.M. Ditto at the sea-shore,   —   30.032   ——   44

Whence we have the following computations, equated for the times corresponding to the two observations at the top.

$$7 \text{ h. } 45' \text{ A.M.} \begin{cases} 30.038 & 46°-046=29.992 \\ 28.266 & 42 \ -031=28.235 \end{cases} = 1573 \ \begin{cases} +70. \\ =44.5 \end{cases} \begin{matrix} 43° \\ 41 \end{matrix} \Big\} 42°$$

$$8 \text{ h. } 45' \text{ A.M.} \begin{cases} 30.036 & 45°-043=29.993 \\ 28.258 & 42 \ -031=28.227 \end{cases} = 1581.3 \ \begin{cases} +78.3 \\ =49.5 \end{cases} \begin{matrix} 43 \\ 41 \end{matrix} \Big\} 42$$

Mean $1577.1 = \dfrac{+47}{1000}$

(Quicksilver to air as 10224 to 1.)

From these observations it appears that, instead of the usual equation $\frac{24.5}{1000}$, to be added to the logarithmic result, in order to obtain the true height in Britain, when the temperature is 42°, there is an excess of $\frac{47}{1000}$: and, instead of the usual ratio of the weight of quicksilver to columns of air, of equal altitude and temperature in Britain, namely about 11200, we have that of 10224 to 1.

Thus air at Spitzbergen feems to be fpecifically hea-. vier, than that affected with the fame heat and preffure in the middle latitudes: whence it follows that, inftead of 32° which is found to be the zero of the fcale about the middle of the temperate zone, we fhall have $\frac{47}{245} =$ 19°.2 + 42° = 61°.2 for the zero at Spitzbergen, within 10° of the North Pole.

It is much to be regretted, that the French academi-cians, when employed in meafuring the degrees of the meridian in Peru, were not fupplied with better barome-ters, and that they made not obfervations at correfpond-ing times; fince the fcene of their operations was un-doubtedly preferable to any other on the furface of the globe, for determining many curious points with refpect to the modifications of the atmofphere in the torrid zone: neverthelefs, by attending diligently to what Mr. BOUGUER [n] hath told us, of the fteadinefs of the baro-meter.

(n) He fays, that at the South Sea, REAUMUR's thermometer, in the morn-ing before Sun-rifing, ftood at 19°, 20°, or 21°; and in the afternoon at 26°, 27°, or 28°. The refpective means correfpond to 76°½ and 92°½ of FAHREN-HEIT, and make the mean heat of the day 84°½. At Quito the temperature continued at 14° or 15°, anfwering to 65°½ of FAHRENHEIT. At the fummits of Coraçon and Pichincha, the thermometer ftood in the morning feveral degrees below freezing, and varied 17° in the heat of the afternoon; whence the mean temperature at thefe higheft ftations, would probably be about 43°½ of FAHRENHEIT. He farther fays, that in the torrid zone, whatever may be the

mean

meter throughout the year; the uniformity of the mean temperature in every affigned ftation; and his mode of computing, by means of the tables of common logarithms, the altitudes of the Cordillero mountains above the valley that extends itfelf between them; it will be no difficult matter to difcover, nearly at leaft, what fort of equation became neceffary; and what were the relative weights of quickfilver and air of the mean temperature, not only in that high region of the atmofphere, but alfo at the level of the fea.

Thus, by infpecting the table of computations, it will appear, that columns of air, whofe bafes were removed fix or eight thoufand feet from the level of the fea, and whofe heights equalled that diftance, when the temperature was $55°$ of FAHRENHEIT, as determined from the mean between the coldeft of the morning and hotteft of the afternoon, the mean logarithmic defect was only $\frac{36.3}{1000}$: whereas, in meafuring heights near the level of the fea, in middle latitudes, the common equation for that temperature is about $\frac{57}{1000}$. The mean ratio of the weight of quickfilver to air, on thefe long columns comprehended refpectively between Carabourou and Quito, and the

mean heat in any affigned ftation, it continues uniformly the fame throughout the year. In this rough eftimation of the temperature in Peru, it feemed unneceffary to examine, whether the true thermometer of REAUMUR was ufed or not; as it could produce no material difference, except at the very hotteft ftations.

fummits

fummits of Pichincha and Coraçon, is that of 16793 to 1. On the altitude of 1534 feet, intercepted between Carabourou and Quito, which fhort fection of the column is about half-way between the level of the fea, and the fummits of the Cordilleros, the mean temperature being 66°, the ratio is that of 15089 to 1: hence it feems probable, that quickfilver would have to the different fections of the general column of air, comprehended between the fea and the top of Coraçon, nearly the following ratios:

	Temp.	
At the level of the South Sea,	$84\frac{1}{2}$	13100 to 1
Half-way from thence to Carabourou,	$75\frac{1}{2}$	14100
At Carabourou, — —	$66\frac{1}{2}$	15100
Half-way from thence to Coraçon,	55	16100
At the fummit of Coraçon, —	$43\frac{1}{2}$	17100
Whereof the mean is, —	65	15100

Mr. BOUGUER tells us, that the barometer in the torrid zone varies not at the fea-fhore above two and a half, or at moft three lines throughout the whole year. At Popayan, its variation is only a line and a half; and at Quito a fingle line. Now let us fuppofe, that an altitude had been meafured with the barometer at the level of

the

the South-fea, where the defcent of quickfilver at the upper ftation was exactly an inch in the mean heat of the day, anfwering to $84°\frac{1}{2}$. On the former fuppofition of the weight of quickfilver to air, the height would be 13100 inches or 1091.7 feet.

Hence $\begin{Bmatrix} 29.930 & 84\frac{1}{2}-169=29.761 \\ 28.930 & 84\frac{1}{2}-169=28.761 \end{Bmatrix} = 890.6$ feet,

the logarithmic refult, which is defective 201.1, or nearly $\frac{226}{1000}$ parts. Now this equation being divided by 2.45 the mean expanfion of air, we have nearly 92° for the difference between $84°\frac{1}{2}$, the temperature of the obfervation, and the zero of the fcale, which reduces it to $-7°\frac{1}{2}$ of FAHRENHEIT. If it fhould be thought that I have fuppofed the air to be too light at the level of the fea under the equator, let it be taken to quickfilver only, at a mean between 13100 and 12672, which feems to have been the ratio of their weights at Genoa, when Mr. DE LUC's temperature was 79°, and we fhall have 12881 inches, or 1073.4 feet of air, for the counterpoife to the inch of quickfilver in the barometer: hence $1073.4-890.6=\frac{182.8}{890.6}=\frac{205}{2.45}=83°.7$, will denote the number of degrees that the zero of the fcale would, in that cafe, be below the temperature of the air, which brings it to within lefs than a degree of the cy[...] FAHRENHEIT. But in middle latitudes the zero [...] and the equation

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·equation, applicable at the level of the fea for the heat of $84°\frac{1}{2}$, is at moft only $\frac{132}{1000}$ inftead of $\frac{205}{1000}$.

Mr. BOUGUER found, that the rule which his expe‑ rience had furnifhed him with, for computing heights with the barometer between the ranges of the Cordil‑ leros, namely, that of deducting $\frac{1}{30}$th part from the number of toifes expreffed by the logarithmic dif‑ ferences, which agrees nearly with the equation $\frac{36.3}{1000}$ which I have made ufe of in the table of computations, would not anfwer when he came to apply it at the level of the fea. He tells us, indeed, that the elafticities of the air, above and below, are there, as well as in Europe, exactly pro‑ portionable to its condenfations; and even, that the in‑ tenfity of the elaftic force, or fpring of the air, is every where equal in all places of the torrid zone that are con‑ fiderably elevated. The real condenfations in each place are proportionable to the weights of the fuperior columns caufing the compreffion; thefe condenfations being in geometrical, the heights are in arithmetical progreffion. But below the fame law doth not take place; becaufe the intenfity of the elaftic force is really confiderably lefs at the level of the fea, than it is at one or two hundred toifes above it, notwithftanding the effect of the heat, which fhould render it greater. It is to be obferved, that Mr. BOUGUER hath not given us the obfervations whereon

he

he founded this laft deduction; and his note on the text, which I apprehend, neverthelefs, conveys his true meaning, is contradictory to it: for there he fays, that the dilatation occafioned by the heat throughout the day, changed the diftribution of the weight with regard to all the places fituated within the Cordilleros, as well as on other mountains, and made the lower fections of the columns contain lefs and the upper fections more air, than they fhould have done, had it been a perfectly elaftic fluid.

Having now mentioned all the barometrical obfervations that have come to my knowledge, tending any way to throw light on this very intricate fubject, it remains to fum up, from the whole, the general principles whereon I have proceeded in conftructing the table of equation for the heat of the air.

It will be remembered, that I have more than once remarked, that in the Britifh obfervations, when the temperature was $52°$, the defect was $\frac{49 \text{ or } 50}{1000}$, the lowermoft barometer ftanding at or near the level of the fea; but in the obfervations on Tinto, a confiderable hill appertaining to the third clafs, whofe bafe is elevated 700 feet above the level of the Clyde at Glafgow, when the temperature was $52°$, I found the equation to be little more than $\frac{45}{1000}$. Again, thefe two facts being compared with

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the aggregate refult of Mr. DE LUC's obfervations, where
the lowermoft barometer ftood about 1300 feet above
the fea, the equation for the fame temperature feemed
not to exceed $\frac{42}{1000}$. Laftly, thefe circumftances being
confronted with the refults of Mr. BOUGUER's obferva-
tions, where the lowermoft barometer ftood from 6000
to 8000 feet above the fea, the mean equation for 55°
was only $\frac{36.3}{1000}$, which gives $\frac{34}{1000}$ for the heat of 52°. Now
thefe Peruvian obfervations, which I believe to be ex-
ceedingly good from the fteadinefs of the barometer in
that part of the world, being fubftituted in lieu of thofe
not yet obtained in our own quarter of the globe, there
feemed to me to be a neceffity for concluding, that the
equation for middle latitudes, with any affigned tempe-
rature above or below the zero of the fcale, diminifhed
as the height of the place above the fea increafed; which
confequently implied, that the magnitude of the loga-
rithmic terms increafed fafter than the dilatations of the
air. But when the comparifon was carried yet farther, and
the obfervations in Peru and at Spitzbergen were fairly
brought into one view, there appeared to be fufficient
grounds for fufpecting, if not abfolutely for concluding,
that there could be no fixed zero for the fcale depending
on the temperature of the air; but that it would change
with the denfity of the atmofphere appertaining to the
latitudes,

latitudes, climates, or zones of the earth, where the ob-
fervations were made. On this fuppofition it was natural
for the mind to form to itfelf fome general hypothefis,
which might ferve to account for the appearances; and
the firft that prefented itfelf was the following: that the
atmófphere furrounding our globe might poffibly be
compofed of particles, whofe fpecific gravities were
really different; that the lighteft were placed at the
equator; and that the denfity of the others gradually in-
creafed from thence towards the poles, where the hea-
vieft of all had their pofition [o].

It is a well known and eftablifhed fact, that in the
middle latitudes, a North or North-eaft wind conftantly
raifes the barometer, and generally higher as its con-
tinuance is longer. The contrary happens when a South
or South-weft wind blows; for I believe it is commonly
loweft when the duration and ftrength of the wind from

(o) It was fuggefted by Dr. GEORGE FORDYCE, that equatorial and Green-
land air might be brought bottled up, and weighed in this country in air of the
refpective temperatures, by means of a curious balance whereof he is poffeffed,
in order to fee whether any difference could be difcovered in their fpecific
gravities. A thought of the fame kind, but more eafily put to experiment,
occurred to Lieutenant GLENIE, of the Royal Artillery, namely, that of
weighing equatorial and polar fea-water. To this gentleman I am obliged for
his affiftance in part of the manometrical experiments, as well as in feveral of
the computations.

that

that quarter have been the greateſt. Thus the North-eaſt wind, by blowing for any length of time, brings into the middle latitudes a maſs of air heavier than that which naturally appertains to the region, and raiſes the barometer above its mean height. The continuance of a South-weſter carries off the heavy air, depoſits a much lighter body in its ſtead, and never fails to ſink the barometer below its mean height: hence, in the middle parts of Europe, there is a difference of about two inches and a quarter between the higheſt and loweſt ſtates of the barometer. But ſuppoſing it to be only two inches, the difference of preſſure ſtill amounts to $\frac{1}{15}$th part of the whole weight of the atmoſphere. Now it is evident from the Peruvian obſervations, that the greateſt fluctuation of the barometer, which is at the level of the ſea, doth not exceed 0.226 of an inch, or $\frac{1}{133}$d part of the whole preſſure; and if the change ſhould be no greater at the poles, which I think not improbable, it follows, that the meaſurement of heights by means of the barometer, in middle latitudes, will be more precarious and uncertain than in the torrid and frigid zones.

Such in general were the firſt ideas which the compariſon of the operations of the barometer with the effects

of

of the North-eaft and South-weft wind *(p)* on that inftru-ment, fuggefted with regard to the different denfities of the atmofphere in the different zones of the earth.

But fince the experiments on the expanfion of moift air have fhewn its elafticity to be fo much greater than that which is dry, I apprehend, that the fimple principle of heat and moifture may fuffice to account for all the phenomena. Thus it is univerfally admitted, that there is a greater degree of humidity and heat in the air, near the earth's furface, than there is in the higher regions of the atmofphere. The elafticity or expanfion of the lowermoft fection *(q)* of every column of air, whether

long

(p) I have been well informed, that in China the North-weft wind raifes the barometer moft, and is highly electrical; it is at the fame time the drieft and the coldeft; and at Canton, under the Northern tropic, there is frequently ice. On the Eaft-coafts of North America the feverity of the North-weft wind is univerfally remarked; and there can fcarcely be a doubt, that the inha-bitants of California, and other parts on the Weft-fide of that great Conti-nent, will, like thofe on the Weft of Europe, feel the ftrong effects of a North-eaft wind. The extraordinary drynefs and denfity of the wind from the North pole, feems therefore to be occafioned by its paffing over the Continent of Europe and Afia on one fide, and that of North America on the other. Thofe who live on the Eaft and Weft-coafts of South America, will find the drieft and coldeft winds come to them refpectively from the South-weft and South-eaft. As the winds feem to be colder, drier, and denfer, in proportion to the extent of land they pafs over from the poles towards the equator, fo they appear to be more moift, warm, and light, in proportion to the extent of Ocean they pafs over from the equator towards the poles. Hence the humidity, warmth, and lightnefs, of the Atlantic wind to the inhabitants of Europe.

(q) Mr. DE LUC feems to have fufpected fomething of this kind towards the

end

long or fhort, will confequently be greater than the up-permoft fection of it; for the heat, by diffolving the moifture, produces a vapour lighter than air, which mix-ing with its particles, removes them farther from each other, increafes the elafticity of the general mafs, and diminifhes its fpecific gravity comparatively more than it doth that of the fection immediately above it, where there is lefs heat and lefs moifture. Hence I infer, that the equation for the air, in any affigned vertical, will gradually diminifh as the elevation of the place above the fea increafes, and that it will vanifh at the top of the atmofphere. This is in fome refpect confirmed by the experiments on the expanfion of rare air; for from them it appears, when the particles are very far removed from each other, by a great diminution of preffure, as is undoubtedly the cafe in the higher regions of the atmo-fphere, they lofe a great part of their elaftic force. Thus the equation, anfwering to any particular temperature, above or below the zero of the fcale, at different heights above the furface, will, I apprehend, be expreffed by the ordinates to a curve of the hyperbolic order, whofe cur-

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end of his 8th chap. *fur les difficultés à vaincre:* and in that which follows, he gives proofs of the lightnefs of vapours with regard to air, faying, that they point out fire to be their vehicle. He afterwards quotes NEWTON with refpect to the lightnefs of a humid atmofphere compared with one that is dry.

vature may be fuppofed to change faft near the furface
of the earth, and differ infenfibly from a ftraight line at
great heights above it.

With regard to the latitudinal equation, the fame
principle of heat and moifture feems to make it proba-
ble, that fuch will become neceffary in operating with
the barometer; for it is well known, that there is a great
degree of humidity in the air between the tropics; and,
on the contrary, that the polar atmofpheres are very dry.
The heat and moifture being greateft at the equator, there
the elafticity or equation will likewife be the greateft at
the level of the fea; and the zero of the fcale will neceffa-
rily defcend to a lower point of the thermometer, than
that to which it correfponds in middle latitudes. As the
elafticity of the air at the level of the fea, or equal
heights above it, with the fame degree of heat, will al-
ways be proportionable to the quantity of moifture dif-
folved in it, therefore it will gradually diminifh from the
equator towards the poles; that is to fay, the zero of the
fcale will afcend in the thermometer, coincide with the
32d degree in the middle latitudes, and, in its motion
upwards, will give the equation to be applied with the
contrary fign in high latitudes. Hence I infer, that every
latitude, climate, or zone, will not only have its particu-
lar zero, but alfo its particular curve, whofe ordinates
<div align="right">will</div>

will always meafure the equations applicable in the re-
fpective fituations. The equatorial curve will probably
change the fafteft, and the others become gradually flat-
ter, as they approach towards the poles, where the
greater, but more uniform, denfity of the atmofphere
may occafion it to differ little from a ftraight line. I ap-
prehend, however, that even at the pole fome fmall di-
minution might be found to take place in the equation,
was it poffible, in that region, to prove it by experiments
at a fufficient height above the level of the fea.

The table of the equation, depending on the heat of
the air, annexed to this paper, is conftructed for middle
latitudes. It extends to temperatures from 12° to 92° of
FAHRENHEIT; and for fituations fo greatly elevated
above the fea, as to make the mean barometer between
the two ftations ftand no higher than 19 inches. As the
equation correfponding to the lower parts of the atmo-
fphere, contained in the right-hand columns, will come
more frequently into ufe than that appertaining to the
higher regions, comprehended in thofe on the left;
therefore, in the firft, it is given for every half; and in
the laft only, for every whole inch of defcent of quick-
filver in the tube.

The equation found in the column of 29 inches, cor-
refponds exactly with the expanfion of air refulting from

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the manometrical experiments; and the ratio of diminu-
tion, in the temperature of 52°, hath been taken from
the Peruvian obfervations, fuppofing it to decreafe uni-
formly $\frac{2}{1000}$ on each inch, or $\frac{16}{1000}$ on the eight inches be-
tween 29 and 21. For the fake of fimplicity, as well as
from the want of fufficient data for afcertaining the
lengths of the ordinates of the curve, the arithmetical
hath been preferred to any progreffive diminution that
might have been adopted, though by this mode the re-
fults would have agreed better with fome of my own, as
well as Mr. DE LUC's obfervations.   In each of the co-
lumns the equations for particular temperatures, com-
pared with that for 12° or 92°, are reciprocally propor-
tionable, fo that the maximum of the rate always cor-
refponds to the fpace between 52° and 72°, as indicated
by the manometer. It will be obferved, that though the
equation in the table is only given for every 10° of dif-
ference of temperature, yet, by the intermediate rates for
fingle degrees in the columns refpectively, and the ratio
of diminution for the height of the mean barometer
above the fea, expreffed in that towards the right-hand,
the equation for any particular temperature may be rea-
dily obtained.   The application of this table makes the
third part of the rule, for meafuring heights with the
barometer. When the mean temperature of the column

of

of air is above 32° of FAHRENHEIT's thermometer in the ſhade, add the equation correſponding to the temperature and height of quickſilver in the mean barometer to the logarithmic altitude; when below 32°, ſubtract the equation from the logarithmic altitude; the ſum in the firſt caſe, and difference in the laſt, gives the real height.

Beſides the table of equation for the air, adapted to the meaſurement of the greateſt acceſſible heights the barometer can poſſibly be applied to in middle latitudes, I have annexed, for the uſe of thoſe who may prefer ſimplicity, and ſtill doubt of the vertical diminution, a thermometrical ſcale of the equation, ſuited to Engliſh and French meaſures, with their reſpective thermometers. It will readily be conceived, that the diviſions, expreſſing the 1000th parts in this ſcale [r], are unequal, ſince they follow the inverſe ratio of the thermometrical compared with the manometrical degrees. Where theſe laſt are the greateſt, as between 52° and 72°, the diviſions expreſſing the equation are the ſmalleſt, becauſe a greater

(r) Any ſcale of this kind, unleſs it had been mechanically divided by a mathematical inſtrument-maker, could not be rendered very exact; and it may be expected, that the imperfections in the original will be augmented in copying by the engraver, notwithſtanding the utmoſt care on his part: wherefore, on the left-hand ſide of the plate, I have annexed the number of degrees and decimal parts of FAHRENHEIT, below the temperature of 91°.88, correſponding to every $\frac{18}{1000}$th parts of the equation, by which means the unequal ſcale may, at any time, be divided with ſufficient accuracy.

number of them correfpond to the fame thermometrical fpace. When the height is required in fathoms, the zero of FAHRENHEIT correfponds to -71.72, and the boiling point to $+412.49$: the fum of the two equations 484.21 is the actual expanfion of common air from the heat of $212°$. When the French toife is made ufe of as the meafure, the zero of the fcale hath been fhewn to coincide with $57°.18$ of FAHRENHEIT, or $+11°\frac{1}{4}$ of REAUMUR. The negative equation $134°.72$ anfwering to $-14°\frac{1}{4}$ of REAUMUR, and the pofitive $349°.49$ correfponding to $+80°$, or the boiling point, being added together, make again 484.21.

In order to convey a more diftinct idea of the effect which heat produces in the dilatation of different kinds of air, compared with quickfilver, along with the fcale for the equation I have placed another, expreffing the actual and relative expanfions, refulting from the mean of the experiments, for every $20°$ of difference of temperature. This fcale is intended to give a comparative view of the manometrical with the thermometrical fpaces, mentioned in the fecond fection.

I fhall now clofe this paper, which hath already greatly exceeded the limits I wifhed to have been able to prefcribe to it, with a few remarks on the error of the rule, perceivable in the tables of computation, and the mea-

fures that fhould, in my opinion, be taken to bring the theory of the barometer to a ftill greater degree of perfection, fuch as I believe it to be really capable of.

By infpection of the tables containing the computations of the Britifh obfervations, it will be feen, that the error of the rule is in general very fmall. In the London clafs it is greateft on Shooter's-hill, making the height five feet too little. In thofe at Taybridge, one of the obfervations on Schihallien gives a defective refult of $29\frac{1}{2}$ feet; but this is eafily accounted for, as it certainly arofe from the fhort time given to the barometer to lofe the heat it had acquired in carrying upwards, thofe deftined to obferve at the fummit arriving there too late, that is to fay, towards the expiration of the whole hour which the inferior barometer had been obferved in. One of the obfervations on Carmichael-hill, though a fmall height, is defective eight feet, which I afcribe to the South-weft wind and humidity of the air. From the fame caufe I would account for the uniform defect in the firft part of the Edinburgh obfervations: in the laft part of thefe, the circumftances having changed, the error hath the contrary fign. In the Linhoufe clafs of obfervations, the fame caufe of defect appears on the 1ft of December, 1775, and on the 20th of November, and 17th of December, 1776. The only refult which I

consider

confider as very irregular, and do not pretend to account for, is that for the height of Moel Eilio, a hill fituated between Carnarvon and Snowdon: the real altitude 2371 feet, is exceeded by the barometrical refult 21 feet, though the circumftances were fuch as, in other cafes, generally make it fall fhort.

At the bottom of the tables of computations I have occafionally fubftituted Mr. DE LUC's equation for the air, in calculating one or more of the greateft heights, that the difference between the two methods might become more obvious. Thus the firft obfervation on Schihallien is defective $67\frac{1}{2}$ feet; the mean of thofe on Tinto 29 feet; Moel Eilio 41 feet; and Snowdon 81 feet.

With refpect to the refults that the rule produces on Mr. DE LUC's heights it will be obferved, that it anfwers very well in the cold obfervations, which, with his rule, were often defective 60 or 70 feet; but gives too much in thofe that are hot. If, however, the whole of thefe hot obfervations had been included, the apparent error would have been lefs; for the mean defect was taken at $\frac{96}{1000}$ for the hotteft temperature, whereas it fometimes amounts to $\frac{110}{1000}$. On the height of the Dole the rule errs in defect; and on the mean of Mr. DE LA CAILLE's obfervations, at the Cape of Good Hope, it exceeds the truth. By fubftituting Mr. DE LUC's equation for the air,

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in the computations of the Dole and Table-hill, the re-
fpective refults are defective 96 and 62.6 feet.

To the Britifh obfervations a table is annexed, con-
taining the barometrical computations of altitudes not yet
determined geometrically. In the chief part of thefe the
inferior barometer ftood at Belmont-caftle, the feat of the
lord privy-feal for Scotland, by whofe directions the cor-
refponding obfervations were made. This table likewife
comprehends Mr. BANKs's obfervations in 1772, for the
height of the South-pap of Jura, above Freeport in the
ifland of Ifla, and thofe he made the fame year, to ob-
tain the height of Mount Hecla, above Hafniford in
Iceland.

Laftly, it is to be obferved, that in the application of
the table, the equation found in the columns $29\frac{1}{2}$, 30,
and $30\frac{1}{2}$, will never come into ufe, except in the mea-
furement of fhort columns of air, whofe bafes ftand at,
or not much above, the level of the fea[s]. In an ifland,

<div align="right">whofe</div>

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[s] Having been accuftomed, from the beginning, to call the ftation of the
inferior barometer the place of obfervation, and to fuppofe the mean height of
its quickfilver to denote the elevation of the place above the fea, for the fake
of fimplicity I adapted the formula to the height of quickfilver in that baro-
meter, and made all the computations in the tables accordingly. But it having
been fuggefted to me, firft by Sir GEORGE SHUCKBURGH, and afterwards by
Mr. DE LUC, that this mode, though the eafieft, was not ftrictly accurate, nor
confiftent with the principles whereon a vertical diminution of the equation for

<div align="right">the</div>

whofe climate is fo very variable as that of Britain, fettled weather fhould be chofen as the beft time for obfervations. With any fudden fall of the barometer, in any affigned ftation below its mean height, it is apprehended that the rule will have a tendency to give defective refults; and the contrary fhould happen when, from the increafed weight of the atmofphere, it rifes much above the mean height.

From what hath been faid in the courfe of this paper, it will be perceived, that though the error of the rule is in general very fmall, yet now and then fuch irregularities do occur as plainly fhew, that fomething ftill remains to be done, in order to perfect the theory of the barometer.

The exiftence, or otherwife, of a latitudinal equation being a point of the greateft confequence, fhould be determined with fo much care as to leave no doubt remaining on that head. And as this can only be effected by differences that are extremely obvious, the obfervations for that purpofe fhould be made at the equator, and as near as poffible to the poles[t]. Peru is no doubt the beft

fituation

the heat of the air was founded, I have fince changed it to the mean barometer, or middle of the column of air intercepted between the two ftations. In this way all the great heights have been re-computed: the fmaller altitudes, not being fenfibly affected by the alteration, continue as at firft.

(t) Some idea may be formed what altitudes on the furface of the globe are

acceffible

fituation on the globe for conclufive equatorial obferva-
tions; but as it would be found very difficult to carry any
fcheme of that kind into execution, fuch as may be more
eafily obtained in our Weft India iflands, which have the
higheft mountains, would be very fatisfactory with re-
fpect to the expanfion and weight of moift air, at dif-
ferent heights above the furface. At the tops of the
mountains in the torrid zone, the obfervations would
always be fufficiently cold; but it would be of ufe like-
wife, to have the coldeft poffible at the level of the fea,
under or near one of the tropics, when the Sun was in
the other.

With regard to obfervations in the frigid zones, Spitz-
bergen feems to be as proper a fituation as any; though
others may no doubt be found in the Northern parts of
the Ruffian empire: and it is prefumed, that the Pe-
terfburg academy would direct the experiments to be
made.

acceffible to man, by confidering the height above the fea of the inferior line
of perpetual fnow. In the middle of the torrid zone it appears, from Mr.
BOUGUER's obfervations, to be elevated 5201 yards, and 4476 about the tro-
pics. In middle latitudes there is everlafting fnow on the mountains at the
height of 3300 yards. In the latitude of 80° North, Lord MULGRAVE found
the inferior line of fnow to be only about 400 yards above the fea: whence we
may conclude, that the furface of the earth, at the pole itfelf, is for ever
covered with fnow.

The

The Peak of Teneriffe, Ætna, the mountains of Auvergne and Rouffillon, as well as Hecla in Iceland, are all very proper for obfervations in intermediate latitudes.

Within the ifland of Great Britain, Ben Nevis feems to be the beft mountain for barometrical obfervations, becaufe of its great height, its vicinity to the fea, and that there is very good ground clofe to its foot (which is rarely the cafe in the Highlands) for the meafurement of the bafe that would be made ufe of in the geometrical operations.

One of the chief caufes of error in barometrical computations, I apprehend, proceeds from the mode (though fimplicity is in its favour) of eftimating the temperature of the column of air from that of its extremities, which muft be faulty [u] in proportion as the height and difference of temperature are great. Where very accurate conclufions are expected, fimultaneous obfervations, at different times of the day, and different feafons of the year, fhould be made with feveral barometers, placed at different heights, each accompanied with a thermometer and manometer. By this method, the progreffion of temperature, as well as the law of diminution of the equation, from the pofition of the inferior barometer above the fea (if fuch diminution doth really take place)

(u) This is taken notice of by Mr. DE LUC.

would

would be obtained with certainty. Suppofing, for in-
ftance, Ben Nevis was divided into four fections, five ba-
rometers, with as many obfervers, would be neceffary.
This number may feem great, but the expence of peo-
ple employed in that way would be inconfiderable. And
if it fhould be judged proper, there could not furely be
any great difficulty in providing reafonable accommoda-
tion for an obferver, who fhould live a whole year at the
top of the mountain, while another made correfponding
obfervations below.

But the perfecting of the theory of the barometer is not
the only advantage that would accrue from a combina-
tion of thefe obfervations; for, while they were carry-
ing on in different climates, or zones of the earth, good
opportunities would offer of determining the refractions,
as well as the force of gravity and figure of the globe,
from the vibrations of the pendulum.

The mean expanfion of common air is already found
to be greater than what was formerly fuppofed; where-
fore the mean refraction will be altered proportionably.
And fince the expanfion of moift air is found to be fo
much greater than that of common air, a larger field for
inquiry and inveftigation is now laid open.

With refpect to the experiments with the pendulum,
Mr. BOUGUER feems to have been the only perfon, fo far

as I know, who hath taken the denfity of the medium
in which it performed its vibrations into the account, and
given us its length at the equator in vacuo. But if we
are to judge of the denfity of the air in the frigid zone
from the barometrical obfervations at Spitzbergen, the
pendulum there muft have loft fo much of its weight, as
to have leffened confiderably the number of vibrations
below what they would have been in vacuo, in the fame
temperature. Having confidered the effect that this
would produce, I collected the beft experiments that
have hitherto been made with the pendulum into one
view, and having applied the equation that the denfity
of the air, in which they feverally vibrated, feemed to
require; I found from computation, that the ratio of the
diameters of the earth is (as Mr. BOUGUER fuppofed it)
nearly that of 178 to 179, inftead of 229 to 230, as
eftimated by Sir ISAAC NEWTON, and which agrees very
nearly with the mean refult from the meafurement of
the degrees of the meridian. The experiments with the
pendulum are fo fimple and eafy, may be repeated fo
often in all fituations, and are fo much more confiftent
with each other, than the meafured lengths of degrees
of latitude, that it appears to be incomparably the beft
method for determining the figure of the earth. And if
it fhould really be found fo flat a fpheroid as the pendu-

lum

lum feems to make it, both parallaxes and refractions, will require correction.

Upon the whole, though I wifhed to be concife in the recital of the experiments and obfervations contained in this paper, yet I found it neceffary at the fame time to be explicit. Some of them were either entirely new, or managed in a different manner from what they had formerly been. This forced me into a comparifon of many minute circumftances attending the operations, and to a tedious, though neceffary, combination of the various refults. Without taking a comprehenfive view of the whole matter, and ftating every thing with fairnefs and candour, I could not convey to others the ideas I entertained of it myfelf; nor enable them to judge, how far I had been juft in the conclufions already drawn, or confiftent in my fuppofitions concerning fuch points as are yet left doubtful. If I have been obliged to differ from Mr. DE LUC, it is becaufe the Britifh obfervations, as well as his own (confidered by their extremes) feem to authorize it: he is himfelf too candid to fuppofe, that I have had criticifm in view, or indeed any other object, than that of contributing my mite towards the difcovery of the truth, from the very good foundation which he hath already laid for it. I am aware it may be alledged, that I have rendered the theory of meafuring heights by

5   the

the barometer fo much more complicate and difficult, as perhaps to deter others from applying it to ufeful purpofes. To this I anfwer, that though it feem utterly impoffible to render what is really intricate in its nature, extremely fimple; yet that the beft and fureft method of arriving at fimplicity at laft will, in the firft place, be to afcertain the limits of deviation of the rule, by a proper number of good obfervations, made in circumftances and fituations as different as poffible from each other. In the prefent ftate of the matter, I doubt not but the barometer will be found to give refults fufficiently near the truth for all ordinary purpofes, the nicer bufinefs of levelling alone excepted. It is the only inftrument by which the relative heights of places, in very great and diftant tracts of country, can eafily and fpeedily be obtained, by the preffure of the atmofphere alone. The method of ufing it is attainable by all, requiring only a little habit, and fome degree of attention to prevent the admiffion of air into the tube. Few people are qualified for the tedious and very laborious operations of accurate geometrical meafurements. Mountainous countries rarely afford bafes of fufficient length, which, to avoid error, muft be meafured again and again with the utmoft care. Inftruments of the moft expenfive kinds muft be employed to take the angles; at the fame time that a thorough knowledge of their ufe,

and

and a fcrupulous attention to their various adjuftments, become indifpenfably neceffary. In fhort, the facility of one method, compared with the other, is fo exceedingly obvious as to need nothing elfe to recommend it as a fubject very curious and ufeful, and therefore well worthy of the refearches of philofophers, till, by their united labours, it hath been brought to perfection.
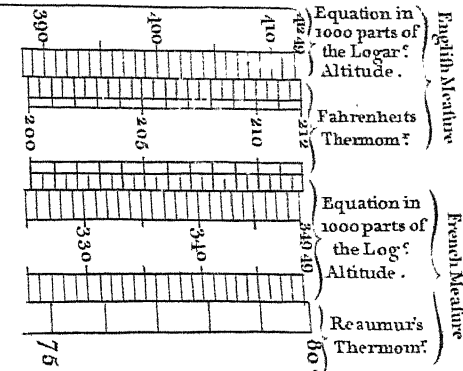
By means of these numbers expressing the Degrees of Fahr. below 212 answering to every 1000 parts of the Equation for the Air, the unequal Scale may be divided.

Subſtract from the *Logarithmic Altitude*.

Add to the *Logarithmic Altitude*.

SCALE for the Equation of the Air, English Meaſure.

Equation in 1000 parts of the Logar. Altitude.

Fahrenheits Thermom.ᵗ

SCALE for the Equation of the Air, French Meaſure.

Equation in 1000 parts of the Logr. Altitude.

Reaumur's Thermom.ᵗ

Subſtract from the *Logarithmic Altitude*.

Add to the *Logarithmic Altitude*.
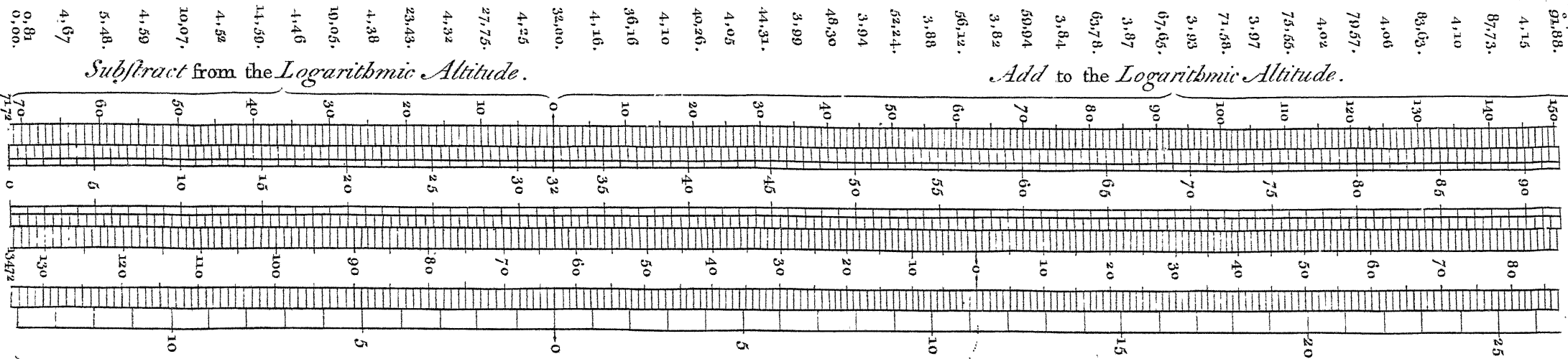
A SCALE expreſſing the Expanſion of different kinds of Air with regard to Mercury affected by 212.° of Fahrenheit.

Actual Expanſion of 1000 equal parts.

Mercury.
17.06

Common Air.
484.21

Very Rare Air.
166.66

Moiſt Air.
2038.06
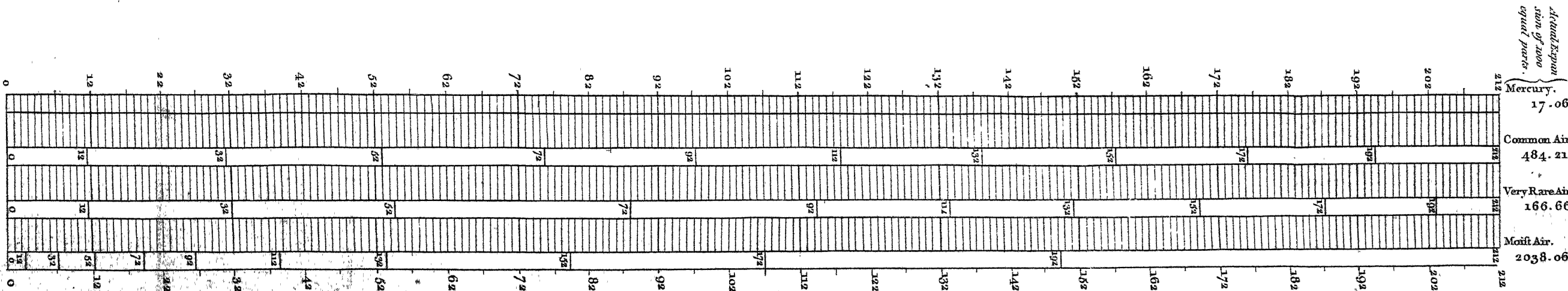
N° I. Computations of barometrical observations made on heights in and near London.

Geometrical heights of the stations in feet.	Date of the observations, winds, &c.	Observed heights of the inferior and superior barometers.	Temperature of the quicksilver.	Equation for the heat of the quicksilver.	Equated heights of the barometers.	Logarithmic result in feet.	Logar. excess or defect in ft. and also in 1000th parts.	Temperature of the air, Particular.	Mean.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Mean.	Error of the rule in feet.	Ratio of the height of quicksilver to air.
St. Paul's Church-yard North-side, and iron-gallery over the dome, 261 feet.	1774, Dec. 1. 9ʰ 27′ wind N.W.	29.659 / 29.338	33¾ / 34	—.006 / —.006	29.653 / 29.332	283.6	+2.6ft, = 9/1000	32¼ / 33¾	33¼	3.8/1000 = 11¼ ft.	284.6			10505
	Dec. 31. 11ʰ 52′ A.M. wind N.E.	30.187 / 29.864	35½ / 34½	—.011 / —.009	30.176 / 29.855	279.5	—1.5 / 5.5	33 / 34	33½	+4. / 1.	280.5	281.4	—0.4	11056
	April 22. 11ʰ 55′ A.M. E. wind.	30.136 / 29.839	50¼ / 53	—.060 / —.069	30.076 / 29.770	266.5	—14.5 / 54.4	49 / 53	51	+47.5 / 14.7	279.2			10860
Top of Paul's-stairs, and the said gallery, 324 feet.	1774, Ap. 22. 0ʰ 50′ P.M.	30.206 / 29.842	55½ / 53¼	—.080 / —.071	30.126 / 29.771	308.9	—15.1 / 49.	55½ / 53¾	54½	+55. / 17.	325.9			9565
	Dec. 1. 10ʰ 2′ A.M.	29.717 / 29.344	37 / 35	—.016 / —.009	29.701 / 29.335	323.1	+0.9 / 3.	37¾ / 34	35½	+6.3 / 2.	325.1	324.6	+0.6	10251
	Dec. 31. 0ʰ 22′ P.M.	30.230 / 29.858	35½ / 34¾	—.011 / —.009	30.219 / 29.849	321.	—3. / 9.3	34¾ / 34¾	34½	+5.8 / 1.8	322.8			
Scotland-yard wharf, and Old Spaniard dining-room, 422 feet.	1774, Dec. 24. 10ʰ 7′ A.M. N.E. wind.	30.844 / 30.349	33¾ / 33¼	—.005 / —.004	30.839 / 30.345	420.8	+1.2 / 3.5	34¼ / 33¾	33¾	+4. / 0.9	421.7		—0.5	10694
	1774, Nov. 28. 9ʰ 48′ A.M.	29.684 / 29.287	35½ / 34¾	—.011 / —.009	29.673 / 29.278	349.2	—2.8 / 8.5	35¾ / 34¾	35	+7. / 2.4	351.6			10404
	Dec. 9. 0ʰ 15′ P.M. N.W. wind, snow,	29.647 / 29.234	27½ / 25¼	+.015 / +.022	29.662 / 29.256	359.	+7. / 19.7	27½ / 23¾	25½	—14.5 / 5.2	353.8			10338
	Dec. 24. 10ʰ 52′ A.M. N.E. wind.	30.758 / 30.343	35 / 33	—.010 / —.003	30.748 / 30.340	348.1	—3.9 / 11.2	34½ / 30¾	32½	+1.2 / 0.4	348.5			11416
Great Pulteney-street, and the said dining-room, 352 feet.	1775, June 13. 11ʰ 7′ A.M. S.W. wind.	30.044 / 29.674	69 / 69	—.121 / —.117	29.923 / 29.557	320.7	—31.3 / 97.6	67½ / 72½	70	+96. / 30.8	351.5			10971
	1776, May 10. 10ʰ 30′ A.M.	30.096 / 29.706	53 / 51¼	—.069 / —.063	30.027 / 29.643	335.4	—16.6 / 50.	51½ / 49¾	50¾	+46.9 / 15.7	351.1			11437
	May 30. 11ʰ 40′ A.M. S.W. wind.	29.900 / 29.521	66 / 63	—.111 / —.100	29.789 / 29.421	323.9	—28.1 / 86.7	66 / 63	64½	+81.8 / 26.5	350.4	353.5	+1.5	11416
	June 20. 0ʰ 15′ P.M.	30.268 / 29.898	71½ / 71½	—.129 / —.127	30.139 / 29.771	320.1	—31.9 / 99.7	71 / 71½	71¼	+101. / 32.3	352.4			11355
	July 16. 0ʰ 15′ P.M.	29.625 / 29.253	67½ / 67½	—.113 / —.112	29.512 / 29.141	329.6	—22.4 / 68.	67¾ / 65½	66¼	+87.4 / 28.8	358.4			10887
	Aug. 26. 10ʰ 35′ A.M.	30.132 / 29.738	59½ / 57¼	—.092 / —.082	30.040 / 29.656	335.2	—16.8 / 50.1	59½ / 56½	57¼	+65. / 21.8	357.0			11028
	Aug. 27. 11ʰ 45′ A.M.	30.020 / 29.631	62¼ / 60	—.099 / —.091	29.921 / 29.540	334.	—18. / 54.	62 / 58¾	60	+70.2 / 23.4	357.4			11355
	Sept. 2. 10ʰ 15′ A.M.	29.294 / 28.918	60 / 58½	—.089 / —.084	29.205 / 28.834	333.1	—18.9 / 56.8	59½ / 59½	59½	+69. / 23.	356.1			
Pagoda in Kew-gardens, 116.5 feet.	1773, Dec. 20. 1ʰ 22′ P.M. mean of 6 observations with 3 barometers.	29.351 / 29.226	49¼ / 49¼	—.052 / —.052	29.299 / 29.174	111.4	—5.1 / 45.8	49¼ / 49¼	49¾	+43.3 / 4.8	116.2		—0.3	11184
Gun-wharf of Woolwich-warren, and upper story of Shooter's-hill inn, 444 feet.	1774, Apr. 27. 4ʰ P.M. mean of 4 obs.	29.762 / 29.282	57¼ / 56½	—.082 / —.077	29.680 / 29.205	420.4	—23.6 / 56.1	52½ / 58½	55½	+59.2 / 24.9	445.3			11170
	Apr. 27. 6ʰ 30′ P.M. mean of 4 obs.	29.773 / 29.302	54 / 55½	—.072 / —.074	29.701 / 29.228	418.3	—25.7 / 61.4	49 / 49½	49¼	+43.5 / 18.2	436.5	438.9	—5.1	11121
	Apr. 28. 5ʰ A.M. mean of 5 obs.	29.805 / 29.336	44½ / 48½	—.041 / —.053	29.764 / 29.283	424.5	—19.5 / 46.	43 / 41¼	42⅛	+24.5 / 16.4	434.9			11077

Nº II. Computations of barometrical observations made on heights near Taybridge in Perthshire, and Nº III. of those near Lanark.

Nº II. near Taybridge.

Geometrical heights of the stations in feet.	Date of the observations, winds, &c.	Observed heights of the inferior and superior barometers.	Temperature of the quicksilver.	Equation for the heat of the quicksilver.	Equated heights of the barometers.	Logarithmic result in feet.	Logar. excess or defect in ft. and also in 1000th parts.	Temperature of the air (Particular)	(Mean)	Equation by the r.'s, and also r.'s in 1000th in feet.	Result by the rule (Particular)	(Mean)	Error of the rule in feet.	
Station at Weem, and top of Weem-craig, 700¼ feet.	1774, July 16. 11h 30' A.M. bright Sun-shine.	29.996 / 29.237	66 0½ / 65¼	–122 / –107	29.874 / 29.130	657.2	–43.2ft. = .653/1000	65 0½ / 60	62 0¼	+27/1000 = 50.0ft.	707.8		+7.6	11249
Ditto station, and top of Bolfrack's cairn, 1076¼ feet.	July 16. 6h 30' P.M. calm and cloudy.	29.933 / 28.788	61¾ / 58½	–097 / –083	29.836 / 28.705	1007.	69.5 / 69.	60 / 56¾	58½	+67.5 / 68.5	1075.5		–1.	11382
Ditto station, and top of Dull-craig, 1244¼ feet.	July 18. 7h 20' A.M.	29.825 / 28.500	58¼ / 55	–086 / –072	29.739 / 28.428	1174.	70.2 / 60.	57 / 55	56	+60. / 70.4	1244.4			11354
Ditto station, and top of Knock-farle, 1364½ feet.	July 18. 5h 4' A.M.	29.816 / 28.347	55¾ / 51	–077 / –059	29.739 / 28.288	1303.5	61. / 46.8	54 / 48½	51¼	+47.5 / 62.	1365.5		+1.	11254
Ditto station, and that in Glenmore, 1279¾ feet.	July 12. 7h 30' P.M.	29.528 / 28.161	58 / 51¼	–084 / –060	29.444 / 28.101	1216.5	62.7 / 51.6	55 / 51¼	53¾	+52. / 63.2	1279.7			11396
Ditto station, and South observatory on Schihallien, 2098 ft.	July 11. 7h 30' P.M.	29.643 / 27.432	53¾ / 48	–086 / –048	29.557 / 27.384	1689.8	108.2 / 54.4	58 / 47¾	52½	+51. / 101.5	2091.3		–6.7	11554
Ditto station, and West summit of Schihallien, 3281 feet.	July 11. 7h 30' A.M.	26.595 / 26.194	59½ / 44½	–089 / –040	29.506 / 26.154	3142.3	138.7 / 44.1	56 / 45	50½	+43.5 / 136.7	3279.		–2.	11693
Ditto station, and West summit of Schihallien, 3281 feet.	July 12. 5h A.M.	29.610 / 26.223	50¾ / 44	–062 / –035	29.548 / 26.188	3145.5	135.5 / 43.1	50¾ / 42	46⅓	+34.3 / 107.	3252.5		–29.5	11690
Station in Glenmore, and the South observatory, 818.76.	July 12. 8h P.M.	28.161 / 27.325	51¼ / 48¼	–060 / –050	28.101 / 27.275	777.4	41.4 / 53.2	51½ / 48	49¾	+42. / 32.6	810.		–8.3	11851

The observation on Schihallien on July 11, by Mr. DE LUC's equation for the air, 3142.3 } 40°.5 –39°.7 := 10°.8 × 2.1 { +22.7 / 71.3 } 3243.6 } –67.4

Nº III. near Lanark.

Geometrical heights	Date	Observed heights	Temp. quicksilver	Eq. heat	Equated heights	Log. result	Logar. excess/defect	Air Particular	Mean	Equation by the r.'s	Result Particular	Mean	Error
Level of the Clyde at Lanark Bridge, and the station at the garden, 362½ feet.	1774, Aug. 20. 6h 30' A.M.	29.776 / 29.383	62½ / 61¼	–099 / –094	29.677 / 29.289	342.9	–19.6 = 57.1	62 / 62	62	+7. = 25.4	368.3	361.4 ±1.9	11683
	Aug. 23. 3h 8' P.M.	29.956 / 29.563	64¾ / 65	–107 / –106	29.849 / 29.457	344.5	18. = 52.3	63 / 63	63	+77.8 = 26.8	371.3		11182
	Sept. 5. 8h A.M.	29.626 / 29.232	52½ / 50½	–067 / –060	29.559 / 29.172	343.4	–19.1 = 55.6	52¾ / 49¼	51	+46. = 15.8	359.2		10875
	Sept. 7. 7h 47' A.M.	29.864 / 29.467	50½ / 51	–061 / –062	29.803 / 29.405	350.3	–12.2 = 34.8	45 / 44	44½	+30.5 = 10.7	361.0		10946
	Sept. 7. 9h A.M.	29.886 / 29.488	50½ / 51¼	–061 / –063	29.825 / 29.425	351.8	–10.7 = 30.4	47 / 41	44	+29. = 10.2	362.0		11430
Level of the Clyde, and Stonebyre-hill, 654 feet.	Sept. 7. 8h 15' A.M.	29.872 / 29.148	48½ / 46¼	–055 / –045	29.817 / 29.103	631.6	–22.4 = 35.4	46¾ / 45¼	45¾	+32.3 = 20.4	652.		–2.0
Carmichael-well, and West-end of Carmichael-hill, 451½ feet.	July 30. 5h 40' P.M. S.W. wind, beginning to rain.	29.162 / 28.690	56 / 54½	–076 / –071	29.086 / 28.619	421.8	–29.7 = 70.3	54¾ / 53¼	54	+52.3 = 22.	443.8		–7.7	11684
	Aug. 1. 11h 40' A.M.	29.612 / 29.135	58¼ / 60	–086 / –089	29.526 / 29.046	427.1	–24.4 = 57.1	57 / 54¾	55⅞	+57.1 = 24.4	451.5		
	June 30. 1h 30' P.M.	28.991 / 27.284	61⅛ / 55¾	–093 / –069	28.898 / 27.215	1563.6	–78.9 = 50.5	58 / 51	54½	+53. = 83.5	1646.9		11412
	July 30. 6h A.M.	29.063 / 27.335	51½ / 46¾	–062 / –043	29.001 / 27.292	1582.6	–50.9 = 38.	51 / 44	47½	+36.4 = 57.4	1640.		11704
Carmichael-well, and top of Tinto, four feet below the summit of the Cairn, 1642.5 ft.	Aug. 2. 8h 15' A.M.	29.608 / 27.846	54⅛ / 47½	–072 / –048	29.536 / 27.798	1580.3	–62.2 = 39.3	51¼ / 44½	48	+39.6 = 62.4	1642.7	1645.5 +3.0	
	Aug. 27. 11h 50' A.M. S.W. wind.	28.710 / 27.008	59¾ / 53¼	–087 / –063	28.623 / 26.945	1579.8	–72.2 = 46.	55¼ / 47¾	51½	+45.6 = 72.3	1643.6		
	Aug. 27. 1h 40' P.M. hail.	28.736 / 27.032	60¾ / 53	–090 / –062	28.646 / 26.970	157.1	–71.5 = 45.5	55⅛ / 50	52¾	+49. = 76.8	1642.6		
	Aug. 27. 1h 50' P.M. hail.	28.716 / 27.010	58⅛ / 52¼	–083 / –061	28.633 / 26.949	1579.5	–6.3 = 40.	55¼ / 49½	51	+46.4 = 73.3	1647.8		

Mean of the observations on Tinto, with Mr. DE LUC's equation for the air, 1574.5 } 51° –39°.7 := 11.3 × 2.1 { +24.8 / 73.3 } 1613.5 } 39.

N° IV. Computations of barometrical observations on heights near Edinburgh.

Geometrical heights of the stations in feet.	Date of the observations, winds, &c.	Observed heights of the inferior and superior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic result in feet.	Excefs or defect in ft. and also in 1000th parts.	Temperature of the air, Particular.	Mean.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Mean.	Error of the rule, in feet.	
Leith Pier-head, and Calton-hill, 344 feet.	1774, Aug. 12. 5ʰ 20′ A.M.	30.086 / 29.704	52¼ / 49¾	—067 / —058	30.019 / 29.646	325.8	18.2ft. =15.3f. / 90 =1000	50¾ / 49	50°	+47/1000 =15.3f.	341.1	—	—	11037
Leith Pier-head, and top of Arthur's Seat, 803 feet.	Aug. 15. 6ʰ 45′ A.M.	29.568 / 29.197	55¼ / 53½	—075 / —068	29.493 / 29.129	323.6	—20.4 / =60.3	54 / 54½	54¼	+54 =17.5	341.1	339.5	—4.5	11761
Leith Pier-head, and Kirk Yetton cairn, 1544 feet.	Aug. 15. 0ʰ 15′ P.M. S.W. wind and rain.	29.625 / 29.282	56¼ / 53¼	—078 / —068	29.547 / 29.214	319.1	—24.9 / =78.	54 / 54½	54¼	+54 =17.2	336.3	—	—	—
Leith Pier-head, and top of Arthur's Seat, 803 feet.	Aug. 15.5ʰ 15′ A.M. S.W. wind and rain.	29.567 / 28.704	55¼ / 51¼	—075 / —062	29.492 / 28.642	762.	—41. / =53.8	54 / 50½	52¼	+50 =38.	800.	—	—3.	11399
Leith Pier-head, and Kirk Yetton cairn, 1544 feet.	Sept. 15. 10ʰ 30′ A.M. S.W. wind.	29.953 / 28.291	57¼ / 52¼	—084 / —063	29.869 / 28.228	1472.5	—71.5 / =48.6	54¼ / 47¼	51	+47 =69.2	1541.7	—	—2.3	11249
Calton-hill, and Kirk Yetton cairn, 1200 feet.	Sept. 15. 1ʰ 15′ P.M. S.W. wind.	29.561 / 28.272	63¼ / 54	—100 / —068	29.461 / 28.204	1136.2	—63.8 / =56.2	56¼ / 48¼	52½	+52 =59.	1195.2	—	—4.8	11410
Level of Hawk-hill ftudy, and bottom of Small-rock, 7.4 ft. below the top of Arthur's Seat, 702.4 feet.	1774, Dec. 1. 2ʰ 45′ P.M.	29.565 / 28.770	35 / 32	—010 / —	29.555 / 28.770	701.5	—0.9 / =1.3	33 / 30½	31¾	—0.5 =0.3	701.2	705.3	—	10724
	Dec. 10. 9ʰ 46′ A.M.	29.494 / 28.687	20½ / 20½	+038 / +037	29.532 / 28.724	722.9	+20.5 / =28.3	20½ / 20½	20½	—26.6 =19.2	703.7		+2.9	11445
	1775, Jan. 26. 1ʰ 35′ P.M.	29.490 / 28.674	26¼ / 24¼	+018 / +026	29.508 / 28.700	723.5	+21.1 / =29.	26 / 23	24½	—17. =12.4	711.1	—	—	10419
	Nov. 10. 11ʰ 30′ A.M.	29.959 / 29.177	38 / 34	—020 / —006	29.939 / 29.171	677.2	—6.8 / =10.	36¾ / 34	35½	+8.6 =5.8	683.	—	—	10646
Bafe of Hawk-hill obfervatory, and bottom of the Small-rock on Arthur's Seat, 684 feet.	Nov. 17. 9ʰ 30′ A.M.	29.543 / 28.769	33¼ / 30¼	—004 / +005	29.539 / 28.774	683.8	— / —	32 / 29¼	30⅝	—3.5 =2.4	681.4	684.5	—0.5	10729
	1776, Jan. 31. 10ʰ 45′ A.M.	30.009 / 29.229	15½ / 24	+056 / +026	30.065 / 29.225	711.7	+27.7 / =39.	14 / 20	17	—35.2 =25.	685.7		+0.5	10184
	July 25. 2ʰ 20′ P.M.	30.157 / 29.427	70¾ / 66½	—125 / —111	30.032 / 29.316	628.8	—55.2 / =87.7	69¾ / 67	68½	+94 =58.2	687.0	—	—	11416
	1775, Dec. 27. 11ʰ 30′ A.M.	29.807 / 28.985	30¾ / 29¾	+004 / +007	29.811 / 28.992	725.9	—4.9 / =6.7	29½ / 29½	29½	+5.7 =4.1	721.8	—	—	10707
Hawk-hill garden-door, and bottom of the rock on Arthur's Seat, 730.8 feet.	Dec. 27. 8ʰ 40′ A.M.	29.778 / 28.945	35¾ / 33	—013 / —003	29.765 / 28.942	730.6	— / —	35¾ / 32¾	34¼	+5.4 =4.	734.6	734.7	+3.9	11342
	1776, Feb. 1. 9ʰ 30′ A.M.	29.883 / 29.032	28¾ / 26½	+011 / +019	29.894 / 29.051	745.4	+14.6 / =19.6	24½ / 26½	25½	+15.4 =11.5	733.9		+3.9	10390
	Aug. 3. 2ʰ 20′ P.M.	30.135 / 29.348	75½ / 72	—141 / —127	29.994 / 29.221	680.4	—50.4 / =74.	72¾ / 69	73¾	+100 =68.	748.4	—	—7.6	11286

In thefe two laft obfervations Mr. DE LUC's equation for the air being fubftituted,

1776, Feb. 1.	745.4	39°.7—25°.5=14°.2 × 2.1	+65.1 =44.3	723.2	—7.6
Aug. 3.	680.4	70°.7—39°.7=31°. × 2.1	+65.1 =44.3	724.7	—6.1

N°. V. Computations of barometrical observations made on heights near Linhouse. and N° VI. of those near Carnarvon in North Wales.

N° V. near Linhouse.

Geometrical heights of the stations in feet.	Date of the observations, winds, &c.	Observed heights of the inferior and superior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic result in feet.	Excess or defect in 1000th parts, and also in feet.	Temperature of the air, Particular.	Mean.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Mean.	Error of the rule in feet.	Ratio of the weight of quickfilver to air, air being 1.
Linhouse, and East-cairn hill, 5 feet below the summit, 1176.6 feet.	1775, Nov 11. 8ʰ A.M. calm and clear.	29.216 / 27.912	32° / 30	+.006	29.216 / 27.918	1184.2	+7.6 ft. =64/1000	32 / 29	30½	+34/1000 =4. ft.	1180.	} 1181.3	+ 4.7	10894
	Nov. 15. noon.	28.941 / 27.632	32 / 27	+.015	28.941 / 27.647	1191.9	+15.3 / 12.8	32 / 26	29	—7. / 8.3	1182.6			10910
Linhouse, and East-cairn hill, 18 feet below the top, 1165.6 feet.	1776, Dec. 17. 2ʰ P.M.	28.990 / 27.688	31½ / 24	+.001 / +.025	28.991 / 27.713	1174.8	+9.2 / 7.9	30¼ / 22	26⅝	—14. / 16.4	1158.4	—	—7.2	10910
	Substituting Mr. DE LUC's equation for the air,				—	39°.7—20°.1=13°.6×2.1		28.6 / 33.6			1141.2	—	—4.4	
Linhouse, and West-cairn hill, 11 ft. below the top, 1178.4ft.	1775, Dec. 1. 1ʰ P.M. high S.W. wind, fog above.	29.250 / 28.003	49 / 45	—.055 / —.042	29.195 / 27.961	1125.3	—53.2 / 47.3	48° / 45	40½	+35. / 39.4	1164.7	—	—13.7	11441
Linhouse, and Corfton hill, 4 feet below the top, 386.5ft.	Dec. 8. 1ʰ P.M. clear and windy.	29.686 / 29.521	41 / 39	—.029 / —.023	29.657 / 29.288	379.7	—6.8 / 18.	40 / 39	39¼	+18. / 6.8	386.5	—	—	10736
Corfton hill, and West-cairn hill, 792 feet.	1776, Dec. 16ʰ 11ʰ A.M. high N. wind, clear weather.	28.580 / 27.714	34¾ / 32	—.009	28.571 / 27.714	793.6	+1.6 / 2.	34¾ / 30	32⅛	+0.3 / 0.2	793.8	—	+1.8	11077
Corfton hill, and East-cairn hill, 776.6 feet.	Dec. 17. 1ʰ A.M. light W. wind.	28.574 / 27.710	32 / 25	+.022	28.574 / 27.732	779.4	+2.8 / 3.6	31 / 23½	27¼	+10.2 / 9.3	770.1	—	—6.5	11048
Linhouse, and Corfton hill, 388.5 feet.	Nov. 20. 1ʰ P.M. snow had fallen, high W. wind.	27.992 / 27.582	35 / 33	—.009 / —.003	27.983 / 27.579	379.	—9.5 / 28.2	33 / 33	33	+2.2 / 0.8	379.8	—	—8.7	11540

N° VI. near Carnarvon.

	Date of the observations, winds, &c.	Observed heights	Temperature of quickfilver	Equation for heat	Equated heights	Logarithmic result in feet	Excess or defect	Temperature Particular	Mean	Equation by rule	Result Particular	Mean	Error	Ratio
Carnarvon Quay, and Moel Eilio, 2371 feet.	1775, Aug. 4. 1ʰ 7′ P.M. rain above, clear below.	29.693 / 27.714	62½ / 54	—.098 / —.066	29.595 / 27.148	2248.8	+122.2 / 54.4	62½ / 51	56¾	+59.6 =134.	2382.8	—	—	11594
	Aug. 8. 0ʰ 7′ P.M. S. wind, and hazy weather above.	30.036 / 27.543	68 / 57	—.118 / —.075	29.918 / 27.468	2226.3	—194.7 / 65.	68¼ / 56	62⅜	+75. =167.	2303.3	} 2391.8	+20.8	11566
	Aug. 8. 2ʰ 7′ P.M. S. wind, weather something clearer.	30.027 / 27.533	69½ / 58¼	—.122 / —.079	29.905 / 27.454	2228.3	—142.7 / 64.	69½ / 57	63¼	+76.8 =171.	2399.3			11704
	Substituting Mr. DE LUC's equation for the air,				60°.8—39°.7=21°.1×2.1	2231.1	+44.3 / 98.8			—	2330.	—	—41.	
Carnarvon Quay, and Peak of Snowdon, 3555 feet.	Aug. 7. 6ʰ 7′ A.M.	30.154 / 26.462	56¾ / 47½	—.081 / —.045	30.073 / 26.417	3377.6	—177.4 / 52.5	56¾ / 45⅝	50¾	+45. =152.6	3530.2	—	—	11646
	Aug. 7. 9ʰ 7′ A.M.	30.165 / 26.468	60 / 49¼	—.092 / —.050	30.073 / 26.418	3376.6	—178.4 / 52.8	60 / 47¼	53¾	+52.5 =177.4	3554.	} 3551.	—	11704
	Aug. 7. 0ʰ 7′ P.M.	30.140 / 26.488	61½ / 60½	—.097 / —.083	30.043 / 26.405	3363.4	—191.6 / 57.	61½ / 54	57⅞	+61. =205.	3568.4			
	Aug. 7. 2ʰ 7′ P.M.	30.144 / 26.478	62 / 53¾	—.099 / —.063	30.045 / 26.415	3355.3	—199.7 / 59.5	62 / 51	56½	+58.5 =196.	3551.3	—	—	
	Aug. 14. 8ʰ 7′ A.M. fog above.	29.984 / 26.271	56½ / 42¾	—.080 / —.031	29.904 / 26.240	3405.9	—149.1 / 43.8	55¼ / 43	49⅛	+40. =136.2	3542.1	} 3546.8	—	11643
	Aug. 14. 9ʰ 7′ fog and rain.	29.978 / 26.279	58½ / 44	—.087 / —.035	29.891 / 26.244	3390.6	—164.4 / 48.5	57¼ / 43½	50¼	+44.3 =150.4	3541.			
	Aug. 14. 10ʰ 7′.	29.972 / 26.280	60 / 44½	—.091 / —.036	29.881 / 26.244	3381.9	—173.1 / 51.2	60 / 44¼	52¼	+48. =162.7	3544.6			11704
	Aug. 14. 11ʰ 7′.	29.974 / 26.280	61½ / 44¾	—.097 / —.037	29.877 / 26.243	3379.4	—175.6 / 52.	61 / 45	53	+50. =169.	3548.4	—	—	
	Aug. 14. 0ʰ 7′.	29.976 / 26.282	62½ / 46½	—.100 / —.042	29.876 / 26.240	3331.5	—173.5 / 176.2	62 / 46	54	+52.3 =176.2	3557.7	—	—	
	Barometrical height of Snowdon from the mean of two days observations,					3379.1	—175.9 / 52.1	53½	—	—	3548.9	—	—6.1	
	Mr. DE LUC's equation for the air,				53°.1—39°.7=13°.4×2.1=28.14	—	—	—	—	—	3474.2	—	—80.8	

Computations of part of Mr. DE LUC's barometrical observations, answering to the coldest and hottest temperatures of the air.

Stations with their geometrical heights in feet.	Date of the observations.	Observed heights of the inferior barometers.	Equation for the heat of the quick-silver.	Equated heights of the barometers.	Logarithmic result in feet.	Logar. excess or defect in ft. and also in 1000th parts.	Temperature of the air, Particular.	Mean.	Reduced.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Mean.	Error of the rule in feet.	Ratio of the weight of quick-silver to air, air to be as 1.
1ft. 230.5	1760, Feb. 9. 8h 30′ A.M.	28.986 / 28.703	–.006 / +.018	28.980 / 28.721	233.9	+3.4 ft. / 145/1000 = 4.4 / 18.7	24¼° / 26¼	25¼	—	154/1000 = 3 oft.	229.9	} 231.8	+1.3	10598
2d. 457.	March 9. 6h A.M.	28.875 / 28.586	–.018 / +.012	28.857 / 28.598	234.9	+ 4.4 / 18.7	27½ / 32	29½	—	4.9 / 1.2	233.7	}	—	10598
3d. 624.5	March 9. 6h 8′ A.M.	28.875 / 28.342	–.018 / +.006	28.857 / 28.348	463.7	+ 6.6 / 14.5	27½ / 31½	29½	—	5.6 / 2.6	461.1	—	+4.1	10732
4th. 776.7	March 9. 6h 15′ A.M.	28.875 / 28.170	–.018 / —	28.857 / 28.170	627.8	+ 4.3 / 7.	27¼ / 34	30½	—	3.5 / 2.3	625.5	—	+1.	10876
5th. 977.2	March 9. 6h 30′ A.M.	28.875 / 28.009	–.018 / —	28.857 / 28.009	777.2	+ 0.5 / 0.7	27½ / 32½	30	—	4.4 / 3.4	773.8	—	–2.9	10978
2d. 457.	March 9. 6h 45′ A.M.	28.875 / 27.798	–.018 / –.005	28.857 / 27.793	978.9	+ 1.7 / 1.7	27¾ / 32	29¾	—	5. / 4.9	974.0	—	–3.2	11000
3d. 624.5	Feb. 9. 9h A.M.	28.997 / 28.470	+.012	28.997 / 28.482	466.9	+ 9.8 / 21.	25¼ / 29	27½	26°	14. / 6.5	460.4	—	+3.	10849
4th. 776.7	Feb. 9. 9h 15′ A.M.	28.997 / 28.298	+.006	28.997 / 28.304	630.3	+ 5.8 / 9.2	28 / 30	29	28	9. / 5.7	624.6	—	—	10814
5th. 977.2	Feb. 9. 9h 30′ A.M.	28.997 / 28.142	—	28.997 / 28.142	780.2	+ 3.6 / 4.5	40½ / 32	36¼	30½	3.5 / 2.7	777.5	—	+2.5	10901
6th. 1298.9	Feb. 9. 10h A.M.	28.997 / 27.931	–.006	28.991 / 27.920	980.8	+ 3.6 / 3.5	40¾ / 37	38¾	31½	1.1 / 1.1	979.7	—	—	10949
7th. 1513.3	Feb. 9. 11h A.M.	29.002 / 27.604	–.011 / –.017	29.002 / 27.587	1303.4	+ 4.5 / 3.5	41 / 36¼	38¾	31½	1.1 / 1.4	1302.	—	+3.1	11024
8th. 1938.9	Feb. 9. A.M.	29.008 / 27.393	–.011 / –.027	28.991 / 26.934	1513.	+ 0.3 / 0.2	41 / 37¾	39⅛	33½	3.3 / 5.	1518.	—	+4.7	11120
9th. 2094.5	Feb. 9. 11h 15′ A.M.	29.002 / 27.366	–.011 / –.021	29.002 / 27.366	1917.7	+ 21.2 / 11.0	43 / 37¼	40⅛	34½	6. / 11.5	1929.2	—	+9.7	11306
10th. 2356.3	Feb. 9. 11h 45′ A.M.	28.992 / 26.771	–.011 / –.021	28.986 / 26.750	2091.8	+ 2.7 / 1.5	43¼ / 39¼	41¼	35	6.6 / 12.6	2104.4	—	+9.9	11241
11th. 2486.3	Feb. 9. noon.	28.986 / 26.494	–.006 / –.021	28.981 / 26.473	2358.6	+ 2.3 / 1.	44¼ / 38¼	41¼	35	6.6 / 15.6	2374.2	—	+17.9	11274
14th. 2922.	Feb. 9. noon.	28.986 / 26.366	–.006 / –.016	28.980 / 26.350	2479.	+ 7.3 / 3.0	44½ / 36	40¾	34½	5.6 / 14.	2493.	—	+6.7	11357
15th. 3119.2	1759, July 15. 4h P.M.	28.759 / 25.950	–.131 / –.105	28.628 / 25.845	2664.8	–25.9 / 96.7	88½ / 74	81	75½	+101. / 269.4	2934.	—	+12.	12541
10th. 2356.3	July 15. 2h P.M.	28.797 / 25.778	–.131 / –.108	28.666 / 25.670	2876.5	–242.7 / 84.5	85 / 68¼	76¾	73½	+96.5 / 300.8	3177.3	} 3154.4	+35.2	12439
Mth. 2486.3	July 15. 3h 30′ P.M.	28.764 / 25.778	–.131 / –.103	28.633 / 25.675	2841.4	–277.8 / 97.7	90½ / 74	82	76	+102. / 290.2	3131.6	—	—	12603
12th. 2752.	1760, July 20. 10h 15′ A.M.	28.775 / 26.499	–.121 / –.117	28.654 / 26.382	2152.6	–203.7 / 94.7	87¼ / 74	80½	75	+102. / 219.6	2372.2	} 2371.2	+14.9	12429
13th. 2877.5	July 20. 3h 15′ P.M.	28.731 / 26.460	–.131 / –.118	28.600 / 26.342	2143.	–213.3 / 99.5	91¼ / 75¼	83½	77	+106. / 227.2	2370.2	—	—	12468
	July 20. 10h 45′ A.M.	28.769 / 26.366	–.121 / –.104	28.648 / 26.262	2266.	–221.3 / 97.7	87½ / 74½	81	75	+101.8 / 230.7	2496.7	} 2499.2	+12.9	12504
	July 20. 5h P.M.	28.726 / 26.327	–.138 / –.116	28.588 / 26.211	2262.	–224.3 / 99.	92½ / 76¼	84¼	77	+106. / 239.7	2501.7	—	—	
	July 20. 11h 30′ A.M.	28.758 / 26.100	–.126 / –.103	28.632 / 25.997	2516.3	–235.7 / 94.	88 / 72¼	80¼	74	+97.6 / 245.6	2761.9	} 2769.6	+17.6	12543
	July 20. 2h 30′ P.M.	28.720 / 26.066	–.138 / –.110	28.582 / 25.956	2511.2	–240.8 / 95.7	92 / 77	84½	77	+106. / 265.2	2777.4	—	—	
	July 20. noon.	28.747 / 25.977	–.126 / –.103	28.621 / 25.874	2629.3	–248.2 / 94.7	89 / 73¼	81¼	75	+101.8 / 265.6	2897.1	} 2895.4	+18.9	
	July 20. 1h 45′ P.M.	28.720 / 25.691	–.131 / –.105	28.859 / 25.856	2618.2	–259.3 / 99.	92½ / 75	83½	77	+106. / 277.5	2895.7	—	—	

Coldest the Sun-rising Observations.   Coldest of the ordinary observations.   Hottest of the ordinary observations on the highest stations.

Mean of the hottest,   81.7 / 75.6

Compu-

Continuation of Mr. DE LUC's barometrical observations.

Stations with their barometrical heights in feet.	Date of the observations.	Observed heights of the inferior and superior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air, Particular.	Mean.	Reduced.	Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule, Particular.	Mean.	Error of the rule in feet.
The Dole, by Sir GEORGE SHUCK-BURGH's meafure, nearly is above the lake of Geneva 4093 ft. Mr. DE LUC's lowermoft barometer was higher than the lake 183 ft. hence the vertical diftance of the barometer, 4210 feet.	1764, July 29. 1ʰ P.M.	28.953 / 24.951	78¾ / 67	—145 / —095	28.808 / 24.856	3845.	—365 ft. / 949⁄1000 = 9⁄1000	77¾° / 64½	71¼	70°	+ 88⁄1000 = 359.8.	4184.8	—	—
	July 29. 1ʰ 30′ P.M.	28.942 / 24.940	78¾ / 65¼	—145 / —090	28.797 / 24.850	3841.	—369. / 96.	78¾ / 65	71½	70	+ 88.2 = 338.8	479.3	4194.	—16.
	1765, July 21. 10ʰ 30′ A.M.	28.698 / 24.640	67 / 59¼	—108 / —075	28.590 / 24.565	3954.	—256. / 64.7	67½ / 51	59¼	58	+ 60. = 237.	4191.	—	
	1764, July 8. 8ʰ A.M.	28.692 / 24.636	71 / 57¼	—121 / —070	28.571 / 24.566	3935.	—275. / 70.	73 / 56	64½	63	+ 77.4 = 285.	4220.	—	
For the barometrical height of Turin above Genoa,	1757, June 2. 6ʰ A.M.	30.077 / 29.817	76 / 76	—142 / —141	29.935 / 29.686	217.7	+ 20. / 92.	—	75¼	73	+ 103. = 22.4	240.1	238.2	+ 0.6
	June 2. 4ʰ½ P.M.	30.088 / 29.846	82½ / 84½	—163 / —169	29.925 / 29.677	216.8	+ 21. / 96.	—	82	79	+ 108. = 23.4	240.2	—	
	June 23. 9ʰ½ A.M.	30.116 / 29.857	79 / 75	—152 / —138	29.964 / 29.719	214.8	+ 22.8 / 107.	—	79	76	+ 104. = 22.3	237.1	—	
Light-houfe of Genoa, 237.6 ft	June 23.5ʰ45′ P.M.	30.041 / 29.796	79 / 79	—152 / —150	29.889 / 29.646	212.7	+ 24.9 / 117.	—	78	75	+ 106.5 = 22.6	235.3	—	
	July 26. 1ʰ P.M.	30.021 / 29.774	83¾ / 83¾	—166 / —164	29.855 / 29.610	214.7	+ 22.9 / 107.	—	81	77	+ 111. = 23.8	238.5	—	
For the barometrical height of Turin above Genoa,		30.019 / +.071	77 —146		29.944				Mean 76					
		30.090				722.6					+ 111.5	803.2		
		29.197 / +.069	77 —141		29.125				77			+ 80.6		
		29.266												
For the barometrical height of Mr. DE LUC's room above Turin,		29.319 / 28.831	72¾ / 72¼	—129 / —126	29.190 / 28.705	436.6			72¾	—	+ 99. = 43.2	479.8		

Mr. DE LUC's room above Genoa,
Surface of the Lake of Geneva in fummer below Mr. DE LUC's room, 1283. 53.3

Surface of the Lake of Geneva above the Mediterranean, 1229.7

By Mr. DE LUC's rule the Lake is elevated above the Sea 1126 French, or 1200 Englifh feet.

In the obfervations on the Dole, if Mr. DE LUC's equation for the air is fubftituted inftead of that refulting from the Britifh obfervations, the barometrical height will be, } 3894 66°.6—39°.7=26°.9×2.1 { + 56.5 = 220. } = 4114.—96.0

Mr. DE LA CAILLE's barometrical Obfervations, Sept. 22, 1751, at the Cape of Good Hope.

Eaft fignal on the table-hill above the fea, 3417	Height of the obfervatory, — 14	30.174 / —.173	66° —111	29.890	— 211.	3192.	58°—39°.7=18°.3×2.1 { + 38.4 = 1244 } 3364.	66°	+ 62.4 = 199.2	3391.2	—	—11.8	11713
Vertical diftance of the barometers in feet, 3403		30.001											
	11ʰ 30′ A.M.	26.502	52 —058	26.444	— 66.	—		50					
Weft fignal above the fea, 3468	Height of the obfervatory, — 14	30.174 / —.197	66 —111	29.866	— 179.	3289.		66	+ 62.4 = 205.2	3494.2	—	+ 40.2	11662
Vertical diftance of the barometers, 3454	0ʰ 30′ P.M.	29.977 / 26.377	50 —053	26.324	— 544.			58 / 50				Mean	11687	Mean 11713
With Mr. DE LUC's Equation for the air, 3426 } 3240	58°—39°.7=18°.3×2.1	+ 38.4 = 1244										—	—62.6	

Compu-

Computations of barometrical observations made on heights that have not been determined geometrically.

Date.	Stations of the barometers.	Observed heights of the inferior and superior barometers.	Temperature of the quickfilver.	Equation for the heat of the quick-filver.	Equated heights of the barometers.	Logarithmic result in feet.	Temperature of the air, Particular.	Mean.	Equation for the heat of the air, in 10000th parts, and in feet.	Vertical distance of the barometers.	Horizontal distance of the barometer in miles.
1773, July 8.	Level of the sea at Invergourie, and Belmont-castle.	29.932 / 29.734	54° / 57	—072 / —081	29.860 / 29.653	181.3	54° / 54	54°	+54.2/1000 = 9.8 f.	191.1	10 1/4
1776, Sept. 12.	Superior barometer, Top of Kinpurney-hill.	29.988 / 28.974	65 / 62	—108 / —095	29.880 / 28.879	887.9	63 / 57	60	+71./=63.1	951.	2 5/8
	Ditto.	30.331 / 29.275	56 1/4 / 51	—080 / —061	30.251 / 29.214	908.9	57 1/2 / 50	53 3/4	+56./=50.8	955.3 / 959.7	2 3/8
	Castle Menzies.	29.756 / 29.674	60 1/4 / 64 1/2	—092 / —104	29.64 / 29.570	82.7	61 / 63	62	+74./= 6.1	88.8	28 1/2
	Top of Farragan.	29.794 / 27.344	63 3/4 / 52 1/2	—102 / —062	29.692 / 27.282	2205.8	65 / 50	57 1/2	+62.4/=137.8	2343.6	29
Sept. 11.	Top of Ben Lawers.	29.800 / 25.830	55 / 38	—075 / —017	29.725 / 25.813	3677.	54 / 36	45	+30./=110.	3787.	42
Sept. 17.	Top of Ben More.	30.000 / 26.148	55 1/2 / 42	—077 / —029	29.923 / 26.119	3542.9	52 1/2 / 37	44 3/4	+31./=109.8	3652.7	53 1/2
Sept. 12.	Top of Ben Gloe.	29.712 / 26.142	62 / 48	—097 / —041	29.615 / 26.101	3291.3	62 / 45	53 1/2	+51./=167.9	3459.2	27 1/4
Sept. 13.	Blair of Athol-lawn.	29.636 / 29.380	60 / 58	—091 / —083	29.545 / 29.297	219.6	58 / 60 1/2	59 1/4	+67./=14.7	234.3	30
Aug. 22.	Top of King's Seat.	29.904 / 28.791	68 / 66 1/2	—116 / —108	29.788 / 28.683	985.	67 / 64	65 1/2	+84.3/=83.	1068.	6 1/4
1775, Sept. 5.	Hill of Barry.	29.870 / 29.345	62 / 56	—098 / —076	29.772 / 29.269	444.	60 / 56	58	+64./=28.4	472.4	4 1/2
Sept. 5.	Dunsinane-hill.	29.784 / 28.913	62 / 59	—097 / —086	29.687 / 28.827	766.	62 / 59	60 1/2	+71.5/=54.8	820.8	7 1/2
1774, Aug. 29 and 30. mean of three observations.	Quay at the new bridge of Glasgow, and station at Lanark.	29.560 / 28.850	55 3/4 / 52 3/4	—077 / —066	29.483 / 28.784	625.2	53 3/4 / 50 1/4	52 1/4	+50.0/=31.3	656 5	22 1/2
1772, Aug. 6. 2h P.M.	Freeport in the island of Isla, 19 feet above the sea, and summit of the South-pap of Jura.	30.224 / 27.642	67 / 57	—114 / —076	30.110 / 27.566	2300.2	60 / 57	58 1/2	+66.3/=152.5	2452.7	4 1/4
1772, Sept. 25. 9h 25' A.M.	Hafniford in Iceland, at the sea-shore, and summit of Mount Hecla.	29.859 / 24.722	49 / 38	—056 / —016	29.803 / 24.706	4886.8	43 / 24	33 1/2	+3.4/=16.6	4903 4	76

The inferior barometer at Belmont-castle.

Computations of Mr. BOUGUER's obfervations in Peru, fuppofing them to have been made at correfponding times, and in the mean temperature of the day, between the coldeft of the morning and hotteft of the afternoon.

Relative heights of the ftations, with refpect to the South-fea,	Stations of the barometers, with their geometrical diftance in feet.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Mean heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Mean temperature of the air.	Equation for the heat of the air in 1000th parts, and in feet.	Refult by the rule in feet.	Error of the rule.	Ratio of the weight of quickfilver to air, Particular	Mean
Heights of the columns of air, whofe bafes ftood at the fea,	15833 { South-fea, / Coraçon,	29.950 / 16.808	84½° / 43½	—169 / —022	29.761 / 16.786	} 23.27	14922.	{ —911 ft. = 61.6/1000	64°	{ + 616/1000 ft. =919. ft.	} 15841.	+ 8.0	14590	
	15564 { South-fea, / Pichincha,	—— / 16.963	84½ / 44½	—— / —024	29.761 / 16.939	} 23.35	14685.6	{ —878.4 = 60.	64½	{ + 62. =920.4	} 15606.	+42.	14517	14553
	9374 { South-fea, / Quito,	—— / 21.403	84½ / 65½	—078 / ——	29.761 / 21.325	} 25.54	8685.5	{ —688.5 = 80.	75	{ + 90. =781.7	} 9467.2	+93.2	13273	
	7840 { South-fea, / Carabourou,	—— / 22.625	84½ / 66½	—084 / ——	29.761 / 22.541	} 26.15	7240.5	{ —599.5 = 83.	75½	{ + 96.5 =698.7	} 7939.2	+99.2	12968	13120
Superior fections of the columns of air, with the diftances of their bafes from the fea, Coraçon, 15833 } Carabourou, 7840 }	7993 { Carabourou, / Coraçon,		66½ / 43½		22.541 / 16.786	} 19.66	7681.6	{ —311.4 = 40.5	55	{ + 35.2 =274.4	} 7952.	—41.	16623	
Pichincha, 15564 } Carabourou, 7840 }	7724 { Carabourou, / Pichincha,		66½ / 44½		22.541 / 16.939	} 19.74	7445.1	{ —278.9 = 37.3	55½	{ + 36.5 =271.7	} 7716.8	— 7.2	16507	16565
Coraçon, 15833 } Quito, 9374 }	6459 { Quito, / Coraçon,		65½ / 43½		21.325 / 16.786	} 19.05	6236.5	{ —222.5 = 35.7	54½	{ + 33.2 =207.	} 6443.5	—15.5	17149	
Pichincha, 15564 } Quito, 9374 }	6190 { Quito, / Pichincha,		65½ / 44½		21.325 / 16.939	} 19.13	6000.1	{ —189.9 = 31.6	55	{ + 34. =204.	} 6204.1	+14.1	16893	17021
	Mean of the four fuperior columns,	- - -	- - -	- - -	- - -	- - -	- - -	36.3/1000	55	- - -	- - -	— —	16793	
Quito, 9374 } Carabourou, 7840 }	1534 { Carabourou, / Quito,	——	66½ / 65½	——	22.541 / 21.325	} 21.93	1445.	{ — 89. = 61.6	66	{ + 61. = 88.	} 1533.	— 1.	15089	

PLAN

OF THE

TRIANGLES

made use of for obtaining

the Geometrical Distance and Altitude

of SNOWDON and MOEL EILIO

with respect to the Sea at

CARNARVON.

Aug.t 1775.

Scale of Feet

Part of Anglesey

SOUND OF MENAI

CARNARVON

Abermenai Ferry

Abermenai Barracks

Coed Allen

Segontium

St Helens

Digod

Foot Hill

Parallel of Carnarvon L 53.9.47

Road by Bethkellert to Festiniog

Meridian of Carnarvon

New Bridge

Gwyay River

Road from Pemmaen

Dinas Dinlle

Moel Eilio

Kefn-Dw

Dolbadarn

Lakes of

Llyn Brits

Llanberis

Pont-y-Cynant

Bettus

Plas-y-Nant

Moel Tryfan

Llyn Quellyn

Mynydd Mawr

Snowdon

GEOMETRICAL OPERATIONS.

The Base AB, situated on a perfectly level Plan, was measured twice in contrary directions, between the Barracks of Abermenai and the bottom of Dinas Dinlle, an ancient British Fortification, on the Sea Shore.

Triangles.	Angles		Sides	Triangles	Angles		Sides	Relative Heights	Feet		Relative Heights	Feet
C.B.F and C.B.N for the prolongation of the Base B C, and distance of N from C.	Obs	C B F	84.10.40	Feet		Obs'd	E B A	79.49.10	Feet	S above B Trigonometrically	3493.2	For the Altitude of Moel Eilio above D, and the Sea at Carnarvon	E above D Trig.r	2183.8
		B C F	42.55.25	B C. 482.2			A B D	31.24.36		Curvature & Refraction	62.3		Curvature & Refraction	16.3
	Hence	B F C	52.53.55				D B E	48.24.34		Height of the Instrument	4.6		Instrument	4.6
	Obs	N B C	98.46.50	N C. 612.5		Obs'd	E D C	86.27.6	D E. 29519.6	S above B	3560.1		E above D	2204.7
		B C N	50.8.10				C D B	0.30.56		C above B Trig.r	97.3		D above Carnarvon Quay	166.3
		C N B	31.4.50				B D B	86.58.1		Instrument	4.6		Vertical distance of Barom.r	2371.0
A C D and D B C, for the distance of D from A B and C; and for the Angles	Obs	D A C	123.17.50	A D. 17169.7	DEB and DAE, For the distance of Moel Eilio from D and A.	Cont	D E B	44.37.25		C above B	101.9		Quay above Neap Tide	13.
		A D C	23.48.30	C D. 27949.7			A D E	111.13.30		C above N Trig.r	114.5		Moel Eilio above the Sea	2384.
	Cont	A C D	30.53.40				D A E	45.40.10½	A E. 39190.2	Instrument	4.6		E above A Trig.r	2381.6
		N C B	30.53.40	B C. 482.2			D E A	28.56.14½		C above N	109.7		Curvature & Refraction	29.1
C B D and B D C.		C B D	148.36.24	B D. 27537.4	C A T, for the distance of the Foot Hill from A.		C A T	122.9.17	A T. 15430	Hence B above N	7.8		Instrument	4.6
		B D C	0.30.56				A C T	29.86.43		Snowdon above the Sea at N	3561.9		Moel Eilio above A	2385.3
	Obs	A B S	92.36.15				A T C	27.53.58		S above D Trig.r	3338.7	For the Altitude of Moel Eilio above A, and the Sea at Carnarvon	T above A Trig.r	178.6
		A B D	32.24.28				C	11.24.37		Curvature & Refraction	47.8		Curvature & Refraction	4.5
D S B for the distance of Snowdon from S and D.		D B S	61.11.29	B S. 57368.2	Angles of Elevation and Depression.	From	B	3.49.7		Instrument	4.6		Instrument	4.6
	Obs	S D C	89.56.50	D S. 50258.3		From	C Dep	1.24.33		S above D	3389.1		T above A	187.7
		C D B	0.30.56			From	D	8.47.64		D above North Angle of Carnarvon Quay by Levelling	166.3		T above Carnarvon Quay by Levelling	186.6
		S D B	90.6.46				E	4.13.33		Vertical distance of the Barometer	2365.4		Quay above A	2.2
	Hence	D S B	28.42.38				T	0.30.47		Quay above High Water N.Tide	13.		Hence the Vertical distance of the Barometer	2363.2
									Height above the Sea	3368.4		Moel Eilio above Neap Tide	2377.1

N.B. The time not admitting of any actual Survey of the Environs of Carnarvon & Snowdon to be made, the Plan is only to be considered as a flight Sketch, intended merely to convey a general idea of the nature of the Country where the Triangles were situated.

Basire Sculp.t

made variable. I added that the fame effect might be produced without the double refraction of the rock cryſtal, with a priſm made of common glaſs, only ſmaller than the aperture of the object-glaſs. The rays which paſs through the priſm would then form an image which would be ſeen out of its natural place; and thoſe which paſs without would give another image, in the ſame place it would have appeared in, if the priſm had not been there.

A few days after, the Abbé FONTANA was told, that the Abbé ROCHON had thought of another micrometer with a priſm of rock cryſtal, which, approaching more or leſs to the focus of the object-glaſs, had the advantage of producing a very great effect, and might be achromatic; that having accordingly had his inſtrument executed, and made ſeveral obſervations with it, he had prepared a paper on the ſubject, to be read at the next meeting

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ajouté que l'on pouroit avoir le même effet fans la double réfraction du criſtal de roche, en faifant un priſme à verre fimple, mais plus petit que l'ouverture de l'objectif. Les rayons qui paffent par le priſme, formeroient alors une image déplacée de fa poſition naturelle, et les autres qui paſſeroient dehors, donneroient l'autre image, à la même place qu'elle auroit, s'il n'y avoit point de priſme.

Quelques jours après on a annoncé à M. FONTANA que M. l'Abbé ROCHON avoit imaginé un autre micrometre à priſme de criſtal de roche, qui, s'approchant plus ou moins du foyer de l'objectif, avoit l'avantage de produire un effet très grand, et de pouvoir être acromatique: qu'ayant fait executer fon inſtrument, et fait avec lui pluſieurs obſervations, il avoit préparé un mémoire

meeting of the Academy. This the Abbé FONTANA immediately told me, and I repeated what I had faid to him the firft time on the effect of this micrometer; adding at the fame time the precife meafure of the fcale for the meafure of the fame effect, and the facility of obtaining the fame thing without the rock cryftal; mentioning likewife other advantages which might be derived from the common glafs prifm not covering the whole aperture of the object-glafs; and, amongft others, that of being able to meafure much greater angles by this means than by the double refraction of the rock cryftal.

The Abbé ROCHON did accordingly read his paper to the Academy, and mention has been made of it in the public prints: he has therefore the merit of having thought of the fame thing, at the fame time with, or perhaps

before

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fur cet objet, pour lire à la prochaine féance de l'Academie. Mr. l'Abbé FON-TANA eut la bonté de m'en avertir immédiatement; ce fut pour lors que je lui répétai ce que j'avois eu l'honneur de lui dire la premiere fois fur l'effet de ce micrometre, en y ajoutant la mefure précife de l'échelle pour la mefure du même effet, et la facilité d'obtenir la même chofe fans le criftal de roche, avec d'autres avantages que l'on pouvoit tirer du prifme à verre fimple, ne couvrant pas toute l'ouverture de l'objectif, et entre autres celui de pouvoir mefurer par ce moyen des angles beaucoup plus grands, que par la double réfraction du criftal de roche.

M. l'Abbé ROCHON a réellement lu à l'Academie fon mémoire, et on en a fait mention dans les gazettes: ainfi il a le merite d'avoir imaginé la même chofe,

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before me, without any knowledge whatever of my ideas on the fubject; he has been the firft who announced it to the world, who had it executed, and who made ufe of it: I have therefore no pretenfions whatfoever on that head; he has the merit of a great difcovery, and aftronomy has the fole obligation of it to him.

But the Abbé ROCHON has only made ufe of the double refraction of the rock cryftal for his micrometer, and I am affured he has faid, that his prifm could give him no more than fix degrees. Now it is well known, that pieces of rock cryftal, large enough and pure enough for thefe purpofes, are extremely rare; befides, the difficulty of working them is great, that fubftance being harder than glafs, and requiring the utmoft attention in cutting, in order to obtain the difference defired between the two refractions. I think therefore, that it will be doing an

effential

dans le même tems, peut être avant moi, et abfolument fans avoir eu aucune con-noiffance de mes idées fur le même objet; de l'avoir annoncé le premier au public, de l'avoir éxécute, et de s'en être fervi le premier: ainfi je n'ai rien à prétendre de ce côté là: il a le merite d'une belle découverte, et l'aftronomie lui en a toute l'obligation.

Mais M. l'Abbé ROCHON n'a employé, pour fon micrometre, que la double refraction du criftal de roche; et on m'a affuré qu'il a dit, que fon prifme ne pouvoit lui donner que jufqu'à fix degrés. On fait bien que les pieces affez grandes de cette matiere, et affez pures, font très rares; outre la difficulté de la travailler, etant plus dure que le verre, et quelle attention il faut avoir pour la bien couper afin d'avoir la différence des deux refractions que l'on veut. Ainfi

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eſſential ſervice, to propoſe another micrometer of common glaſs, to explain the theory of it, and to extend it to much larger angles, which may render it applicable to the optical inſtruments made uſe of in the navy, in taking geographical latitudes and longitudes.

I had already made a priſm of this ſort, and ſhewed the Abbé FONTANA its effect for the double image of the Sun on his excellent little achromatic glaſs: the two images were procured by applying this priſm to the object-glaſs with the hand, in ſuch a manner that it covered only one-half of it: puſhing it more or leſs forward, occaſioned a change in the brightneſs of the light of the two images, and ſhewed that they might be made equally clear. By changing the inclination of this piece, the diſtance between the two images was varied, which did not alter

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je crois rendre un ſervice encore plus conſiderable, en propoſant cette autre eſpece de micrometre à verre ſimple, en developpant ſa théorie, en l'etendant aux angles beaucoup plus grands, ce qui donne le moyen de l'appliquer auſſi aux inſtrumens d'optique, que la marine doit employer pour obſerver les latitudes et longitudes géographiques.

J'avois déja fait faire un priſme de cette eſpece; et fis voir au même M. l'Abbé FONTANA ſon effet pour la double image du Soleil, ſur ſon excellente petite lunette acromatique: on avoit les deux images, en l'appliquant à la main ſur l'objectif, de maniere qu'il n'en couvroit que la moitié. En le pouſſant plus ou moins avant, on changeoit la vivacité de la lumiere des deux images, où l'on voyoit qu'on pouvoit les réduire à une clarté egale; en variant l'inclinaiſon de cette piece on varioit la diſtance des deux images, qui n'avoit aucune varia-

when its diftance from the object-glafs was varied with-out the glafs. This piece was a common prifm, which gave a refraction a little greater than the apparent dia-meter of the Sun: I added another to it afterwards, of the fame kind and equal, both of them having circular bafes. Turning one of the two parts upon its axis, will vary the angle from o, to double each in particular, which occafions the two images to approach to, or recede from, each other. A much flower variation is obtained by the greater or lefs diftance of the prifm from the object-glafs; but there is a particular reafon for which one cannot give it too large a one, the contraction of the pencil of rays belonging to each point of the object, not allowing that diftance to be very great, for fear of weakening too much the direct image towards the middle of the field, by the

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tion en changeant, hors de la lunette, fa diftance à l'objectif. Cette piece etoit un feul prifme fimple, qui donnoit une refraction un peu plus grande que le diametre apparent du Soleil: j'y ai fait ajouter après un autre femblable et egal; l'un et l'autre ayant les bafes circulaires: en tournant fur fon axe l'une des deux parties, on variera l'angle depuis zero, jufqu'au double de chacun en parti-culier, ce qui fait approcher et eloigner les deux images entre elles: on obtient une variation beaucoup plus lente par l'eloignement plus grand ou plus petit du prifme à l'objectif; mais il y'a une raifon particuliere pour laquelle on ne peut pas lui en donner un trop grand; car le retreciffement du pinceau de rayons appartenans à chaque point de l'objet, ne permet pas de l'en eloigner trop, ce qui, vers le milieu du champ, affoibliroit trop l'image directe, en interceptant une trop grande

interception of too great a part of the fame pencil, which in the end would occafion its being altogether loft.

I have in hand, making for me, a rude machine in which one of the pieces may be turned by the hand upon its axis, to make the diftance between the two images fomewhat larger than that which is intended to be mea-fured, as for inftance the diameter of the Sun; and by the help of a moveable fcrew, one may carry the prifm, thus compofed, to a diftance from the object-glafs, by a motion fimilar to that of the fmall mirror of the telefcope. I have had it adapted to an ordinary glafs of about four feet, where its effect, for the Sun's diameter, muft be much greater than an inch of motion in a minute; for the other planets one may have ten or fifteen lines in a fecond, or even more. Generally the fcale is the whole length of the glafs for the total refraction of the prifm, which like-

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grande partie du même pinceau, et à la fin la feroit perdre totalement.

Je fais faire actuellement une machine groffiere, dans laquelle on peut tourner une des deux pieces à la main, fur fon axe, pour rendre la diftance des deux images un peu plus grande que celle que l'on veut mefurer, comme du diametre du Soleil; et à l'aide d'une vis de rapel, on peut eloigner le prifme, ainfi compofé, de l'objectif, par un mouvement femblable à celui du petit miroir des telefcopes. Je l'ai fait adapter à une lunette ordinaire de près de quatre piés, où fon effet, pour le diametre du Soleil, doit être de beaucoup plus d'un pouce de mouvement par minute; et pour les autres planetes ou peut avoir 10 ou 15 lignes par feconde, et plus encore. Généralement l'échelle eft toute la longuer de la lunette pour

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wife is the cafe in the Abbé ROCHON's prifm, for the dif-
ference of the two refractions.   But one may vary the
angle by applying the prifm without the glafs near the
object-glafs, and turning one of the parts upon its axis.
In that cafe, the fcale of the excefs of the fum of the re-
fractions of the two parts of the prifm above the difference,
will be in length no more than half the circumference of
a circle, though the circle may be made as large as one
pleafes; but the difference of the diftance of the images,
will not be proportional to the difference of the arcs run
through by the index.   In order to determine the relation
which the motion of the index bears to the variation of
the diftance between the two images, one muft have the
folution of a geometrical problem, which is eafily gained
by fpherical trigonomety; but it will be always better to
deter-

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la refraction totale du prifme, ce qui eft le même pour la difference des deux
réfractions dans le prifme de M. l'Abbé ROCHON.   Mais on peut varier l'angle
en appliquant le prifme hors de la lunette, à côte de l'objectif, en faifant tourner
une des deux parties fur fon axe.   Alors l'échelle de l'excès que la fomme des
réfractions des deux parties du prifme a fur la difference, n'aura pour fa longueur
que la demi circonference d'un cercle, quoiqu'on puiffe faire ce cercle auffi grand
que l'on veut; mais la difference de la diftance des images ne fera pas propor-
tionelle à la difference des arcs parcourus par l'index.   Pour déterminer la réla-
tion du mouvement de l'index avec la variation de la diftance des deux images,
il faut réfoudre un probleme de géométrie, et j'en ai la folution bien fimple par
la trigonométrie fphérique; mais il vaudra toujours beaucoup mieux déterminer

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determine this relation by an actual terreſtrial obſer-vation of a divided ruler, obſerved at a given diſtance.

When the angle happens to be a large one, the co-lours would naturally be ſuch as would greatly deform one of the two images of the object, namely, that given by the rays that paſs through the priſm; but this is eaſily remedied, at leaſt in a great meaſure, by making each priſm of two pieces, one of common and the other of flint-glaſs. One may multiply the compoſed achroma-tic priſms with variable angles by making the one give degrees from 5 to 5, or from 2 to 2, and the other the minutes. One may put two on the outſide near the ob-ject-glaſs, which will change the diſtance of the images by the circular motion, and give the angle required a little larger than the real one; and another within which

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ce raport par une obſervation actuelle terreſtre d'une regle diviſée, et obſervée â une diſtance donnée.

Quand il s'agit d'un grand angle, on auroit des couleurs qui déformeroient beaucoup une des deux images de l'objet, c'eſt à dire celle qui eſt donnée par les rayons paſſés à travers du priſme: on les evite aiſément, au moins en grande partie, en compoſant chaque priſme de deux pieces, une de verre commun, et l'autre de *flint-glaſs.* On peut multiplier les priſmes compoſés acromatiques et à angles variables, en faiſant que l'un donne les degrés de 5 en 5, ou de 2 en 2, et l'autre les minutes: on peut en mettre deux dehors, près de l'objectif, qui changeront la diſtance des images par le mouvement circulaire, et donneront l'angle cherché un peu plus grand que le véritable; et un autre dedans, qui

<div align="right">donnera</div>

will exactly give the feconds. I have already thou~~~ ~~ of
the inftruments requifite for obtaining all thefe c~~~ ~~ts
with accuracy, as well as for the application of a v~~ ~~able
prifm to the common failor's octant, and have by me the
folution of the neceffary problems: this will be the ob-
ject of a work I am preparing. In the mean time I pub-
lifh this, to give others an opportunity of hitting on
fomething better concerning the mechanical conftruc-
tion of thefe inftruments.

donnera, avec toute précifion, les fecondes. J'ai déja imaginé les inftrumens
néceffaires pour avoir avec exactitude tous ces objets, comme auffi pour l'appli-
cation d'un prifme variable à l'octant de marine ordinaire, ayant auffi la folution
des problemes neceffaires. Tout cela fera l'objet d'un ouvrage que je prépare
fur cette matiere. En attendant je publierai dans les differens journaux ce
Profpectus, pour donner plutôt à tout le monde le moyen d'imaginer fur la
forme mechanique des inftrumens, quelque chofe de mieux que ce qui m'eft
venu dans l'efprit fur ce fujet nouveau, et bien intereffant.
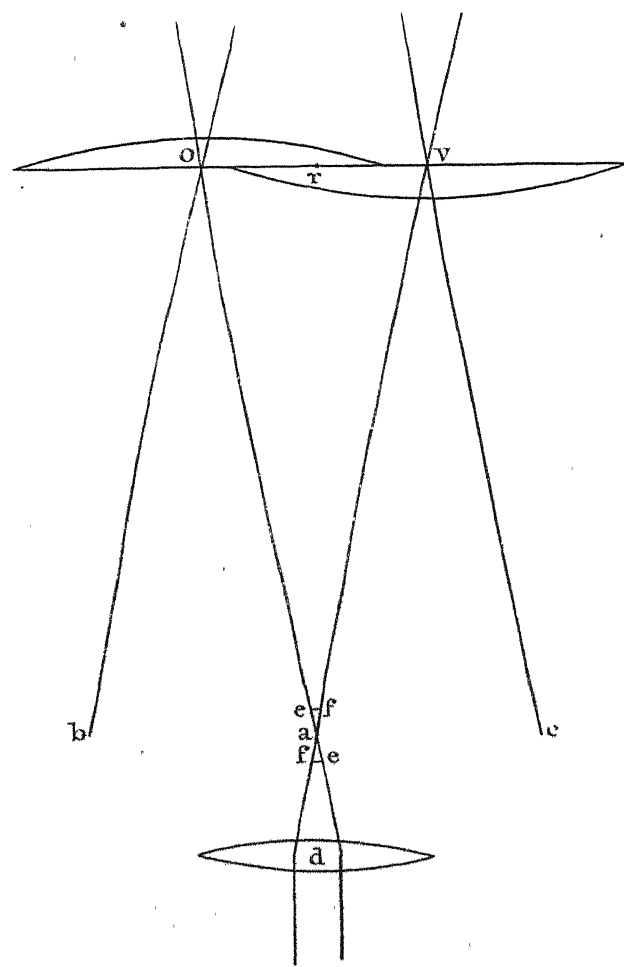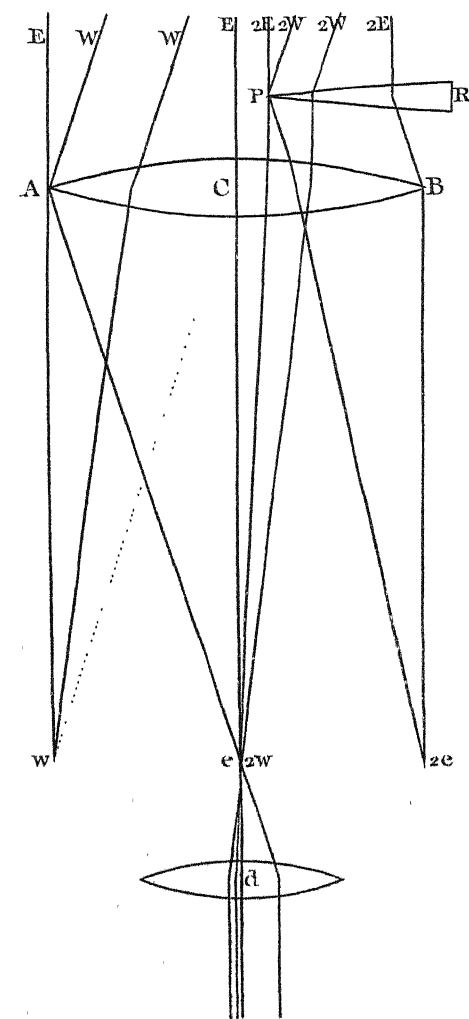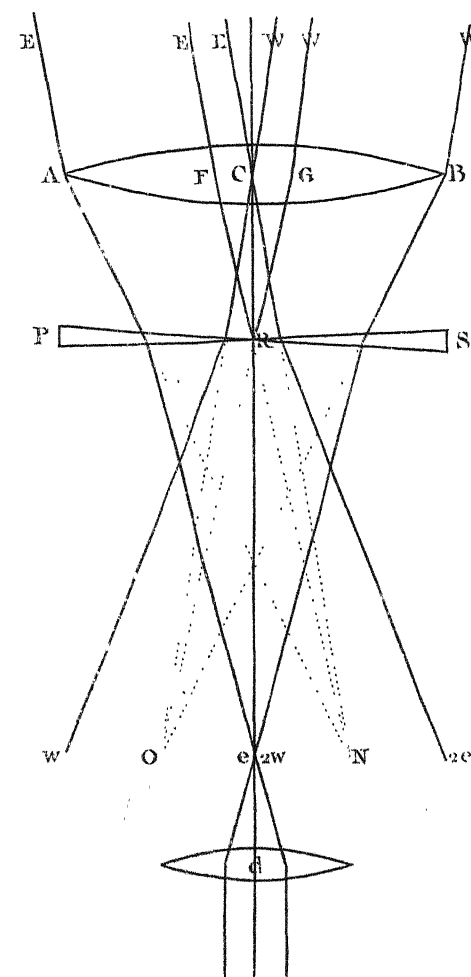
Fig:I.

Fig:II.

Fig:III.
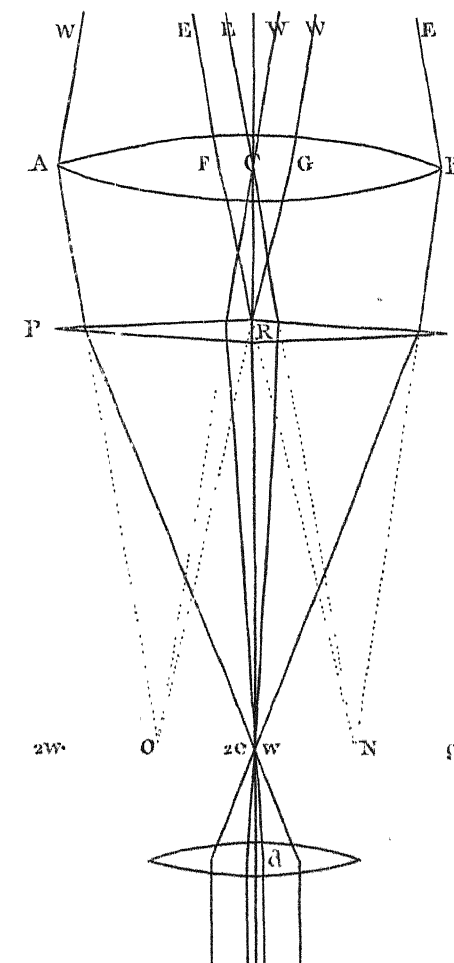
Fig:IV.

XXXVI. *Account of a new Instrument for measuring small Angles, called the prismatic Micrometer. By the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer-Royal.*

PRACTICAL aftronomy was much benefited by the invention of the wire micrometer, for meafuring differences of right afcenfion and declination: nor did it receive lefs advantage from Mr. SAVERY's moft ingenious invention of the divided object-glafs micrometer, which has been rendered more commodious by the late Mr. JOHN DOLLOND's application of it to the object-end of a reflecting telefcope, or the prefent Mr. PETER DOLLOND's application of it to the object-end of an achromatic refracting one.

But, valuable as the object-glafs micrometer undoubtedly is, fome difficulties have been found in the ufe of it, owing to the alterations in the focus of the eye, which are apt to caufe it to give different meafures of the fame angle at different times. For inftance, in meafuring the Sun's diameter, the axes of the pencils of rays, which come through the two fegments of the object-glafs from

4 contrary

contrary limbs of the Sun, croffing one another at the focus of the telefcope under an angle equal to that of the Sun's diameter, the union of the limbs of the two images of the Sun cannot appear perfect unlefs the eye be difpofed to fee objects diftinctly which are placed at point of interfection. But if the eye be difpofed to fee objects diftinctly, which are placed nearer the object-glafs than the interfection is, the two limbs will appear feparated by the interval of the axes of the pencils in that place; and if the eye be difpofed to fee objects diftinctly, which are placed farther from the object-glafs than the interfection is, the two limbs will appear to encroach upon each other by the diftance of the axes of the pencils, after their croffing, taken at that place.

To explain this, let o v (plate XIX. fig. 1.) reprefent the centres of the two femi-circular glaffes of the object-glafs micrometer, feparated to the diftance o v from each other, fubtending the angle o *a* v, equal to the Sun's diameter, at the point *a*, which is the common focus of the two pencils of rays having o *a* and v *a* for their axes, namely, thofe proceeding from contrary fides of the Sun, and paffing through the contrary femi-circles; and let *d* be the eye-glafs. It is evident, that if *d* be properly placed to give diftinct vifion of objects placed at the point *a*, the rays o *a*, v *a*, as well as all the other rays belonging

5   to

Fig: 1.

Fig: 2.

Fig: 3.

Fig: 4.

Fig: 5.

Fig: 6.

Fig: 7.

Interval between Boiling & Freezing in Inches

	1st method	2d method
	31.0	30.7
	29.8	29.5
	28.6	28.3

height of Barometer

Basire Sc.
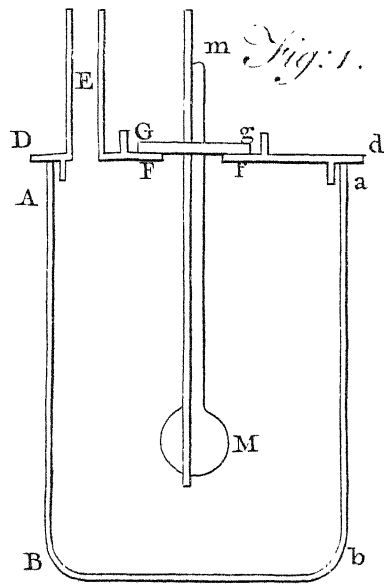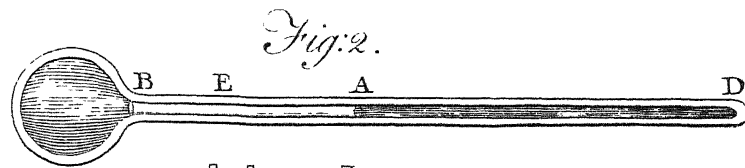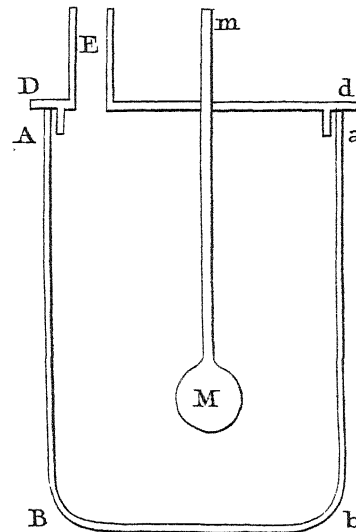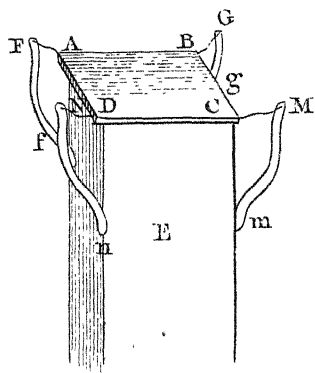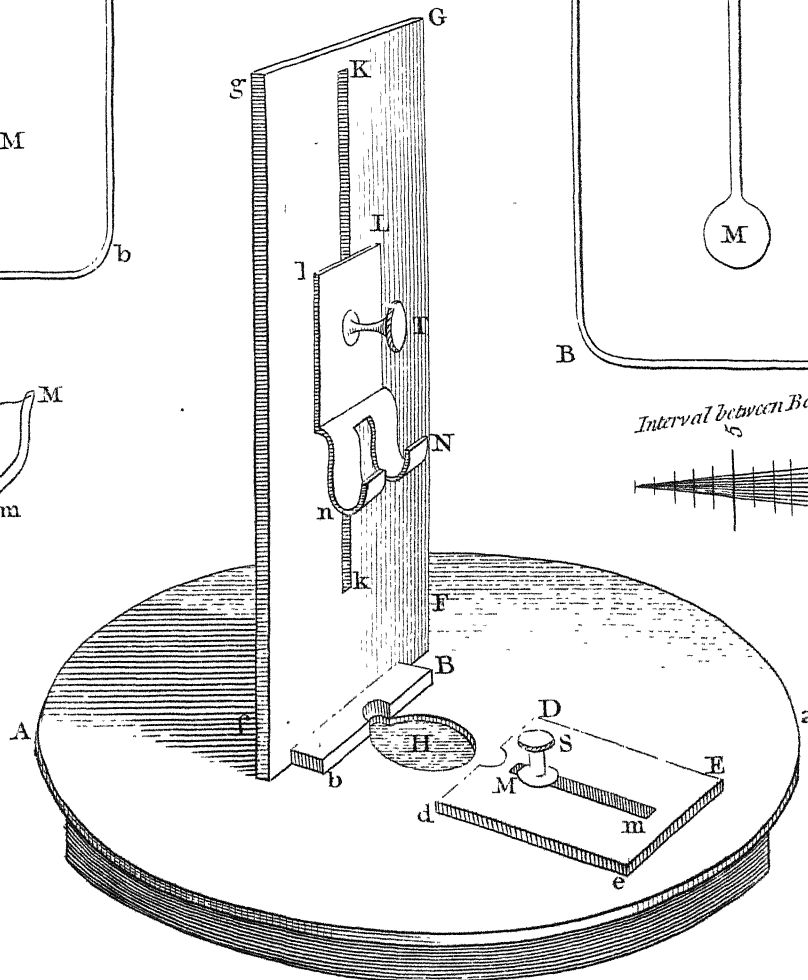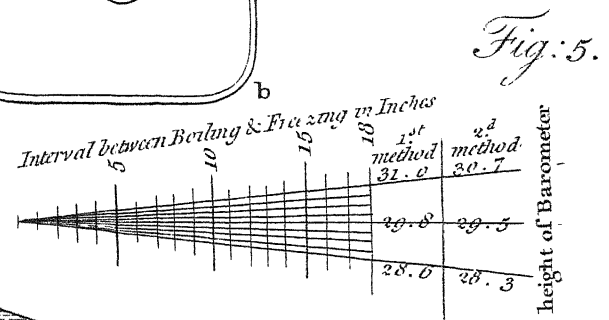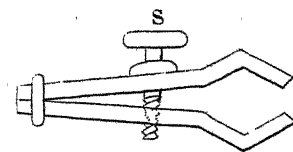
to thofe pencils, will be collected into one point upon the retina of the eye; and confequently, the two oppofite limbs of the two images of the Sun will feem to coincide, and the two images of the Sun to touch one another externally. But if the ftate of the eye fhould alter, the place of the eye-glafs remaining the fame, the eye will be no longer difpofed to fee the image formed at the point a diftinctly, but to fee an object placed at ef, nearer to or farther from the object-glafs diftinctly; and therefore an image will be formed on the retina exactly fimilar to the fomewhat confufed image formed by the rays on a plane perpendicular to their courfe at ef. Confequently, as the two cones of folar rays, boa, cva, formed by the two femi-circles, are feparated or encroach upon one another at this point of the axis by the diftance ef, the two images of the Sun will not feem to touch one another externally, but to feparate or to encroach upon one another by the interval ef. The error hereby introduced into the meafure of the Sun's diameter will be the angle erf, fubtended by ef at r the middle point between o and v, which is to eaf or oav, the Sun's apparent diameter, as ae to cr, or even to ar, on account of the fmallnefs of ae with refpect to ar.

Thefe confiderations concerning the caufe of a principal error that has been found in the object-glafs micro-

meter led me to inquire, whether fome method might
not be found of producing two diftinct reprefentations of
the Sun, or any other object, which fhould have the axes
of the pencils of rays, by which they are formed, diverg-
ing from one and the fame point, or nearly fo: and it
occurred to me, that this might be done by the refraction
of a prifm placed to receive part of the rays proceeding
from the object, either before or after their refraction
through the object-glafs of a telefcope. If the prifm be
placed without the object-glafs, the rays that are re-
fracted through it will make an angle with the rays
that pafs befide it equal to the refraction of the prifm;
and this angle will not be altered by the refraction of the
object-glafs afterwards. Confequently, two images of an
object will be reprefented, and the prifm fo applied will
enable us to meafure the apparent diameter of any ob-
ject, or any other angular diftance which is equal to the
refraction of the prifm. But if the prifm be placed
within the object-glafs, that is to fay, between the object-
glafs and eye-glafs, the angle meafured by the inftrument
will vary according to the diftance of the prifm from the
focus of the object-glafs, bearing the fame ratio to the
refraction of the prifm, as the diftance of the prifm from
the focus bears to the focal length of the object-glafs.

Let

Let ACB (fig. 2.) reprefent the object-glafs, and *d* the eye-glafs of a telefcope, and PR a prifm placed to intercept part of the rays coming from an object, fuppofe the Sun, before they fall on the object-glafs. The rays EE proceeding from the Eaftern limb of the Sun, and refracted through the object-glafs ACB without paffing through the prifm, will form the correfponding point of the Sun's image at *e*; and the rays ww proceeding in like manner from the Weftern limb of the Sun will be refracted to form the correfpondent point of the Sun's image at *w*. But the rays 2F, 2E, 2W, 2W, proceeding in like manner from the Eaftern and Weftern limbs of the Sun, and falling on the prifm PR, and thence refracted to the object-glafs ACB, will, after refraction through it, form the correfpondent points of the Sun's image at 2*e*, 2*w*. Let the refraction of the prifm be equal to the Sun's apparent diameter: in this cafe, at whatever diftance the prifm be placed beyond the object-glafs, the two images of the Sun *we*, 2*w*2*e*, will touch one another externally at the point *e*2*w*; for the rays 2W, 2W, proceeding from the Weftern limb of the Sun being inclined to the rays EE proceeding from the Eaftern limb in the angle of the Sun's apparent diameter, will, after fuffering a refraction in paffing through the prifm equal to the Sun's apparent diameter, emerge

from

from the prifm and fall upon the object-glafs parallel to the rays EE, and confequently will have their focus 2*w* coincident with the focus *e* of the rays EE, and therefore the two images of the Sun *we*, 2*w*2*e*, will touch one another externally at the point *e*2*w*, and the inftrument will meafure the angle EC2W, and that only.

But if the prifm be placed within the telefcope, the angle meafured by the inftrument will be to the refraction of the prifm as the diftance of the prifm from the focus of the object-glafs is to the focal diftance of the object-glafs: or if two prifms be ufed to form the two images, with their refracting angles placed contrary ways, as reprefented in fig. 3. and 4. the angle meafured will be to the fum of the refractions of the prifms as the diftance of the prifms from the focus of the object-glafs is to the focal diftance of the object-glafs. For let ACB (fig. 3.) reprefent the object-glafs, and *d* the eye-glafs of a telefcope, and PR, RS, two prifms interpofed between them, with their refracting angles turned contrary ways, and the common fections of their refracting planes touching one another at R. The rays proceeding from an object, fuppofe the Sun, will be difpofed, by the refraction of the object-glafs, to form an image of the Sun at the focus; but part of them falling on one prifm, and part on the other, will be thereby refracted contrary ways, fo as to form two equal images *we*, 2*w*2*e*, which,

if

if the refractions of the prifms be of proper quantities, will touch one another externally at the point *e2w*. Let ECN be the axis of the pencil of rays EE proceeding from the Sun's Eaftern limb; and wco the axis of the pencil of rays ww proceeding from the Sun's Weftern limb; and the point. N the place where the image of the Sun's Eaftern limb would be formed, and the point o where that of the Weftern limb would be formed, were not the rays diverted from their courfe by the refractions of the prifms. But by this means part. of the rays EE, which were proceeding to N, falling on the prifm PR, will be refracted to form an image of the Sun's Eaftern limb at *e*, while others of the rays EE, which fall on the prifm RS, will be refracted to form an image of the Sun's Eaftern limb at *2e*. In like manner, part of the rays ww, which were proceeding to form an image of the Sun's Weftern limb at o, falling on the prifm RS, will be refracted to form an image of the Sun's Weftern limb at *2w* coincident with *e*, the point of the image correfpondent to the Sun's Eaftern limb; while others of the rays ww, which fall on the prifm PR, will be refracted to form the image of the Sun's Weftern limb at *w*. The two images *we*, *2w2e*, are fuppofed to touch one another externally at the point *e2w*. The ray EFR, which belongs to the axis ECN, and is refracted by the prifm PR to *e*, undergoes the refraction NR*e*, which (be-

caufe

cause small angles are proportional to their sines, and the sine of NR*e* is equal to the sine of its supplement NRC) is to NCR as NC or C*e* is to NR or R*e*. In like manner, the ray WGR, which belongs to the axis WCO, and is refracted by the prism RS to 2*w* or *e,* undergoes the refraction OR*e,* which is to OC*e* as OC or C*e* is to RO or R*e*; therefore, by composition, ORN the sum of the refractions OR*e,* NR*e,* is to OCN the sum of the angles OC*e,* NC*e,* or the Sun's apparent diameter, as C*e* to R*e*; that is, as the focal distance of the object-glass to the distance of the prisms from the focus of the object-glass.

Or let the prisms PR, RS, be placed with their refracting angles P, S, turned from one another as in fig. 4.: the refraction of the prism PR will transfer the image of the Sun from ON to *we,* and the refraction of the prism RS will transfer the image ON to 2*w*2*e,* the two images 2*w*2*e, we,* touching one another externally at the point 2*ew.* Let ECN, WCO, be the axes of the pencils of rays proceeding from the two extreme limbs of the Sun, and N, O, the points where the images of the Sun's Eastern and Western limbs would be formed by the object-glass, were it not for the refraction of the prisms; the ray EFR, which belongs to the axis ECN, and is refracted by the prism RS to 2*e,* undergoes the refraction NR2*e*; and the ray WGR, which belongs to the axis WCO, and is refracted

by

by the prifm PR to w, undergoes the refraction ORw. Now NC2e, part of the angle meafured, is to NR2e, the refraction of the prifm RS, as Rw to Cw; and OCw, the other part of the angle meafured, is to ORw, the refraction of the prifm PR, in the fame ratio of Rw to Cw: therefore OCN, the whole angle meafured, is to ORN, the fum of the refractions of the two prifms, as Rw to Cw; that is, as the diftance of the prifms from the focus of the object-glafs to the focal diftance of the object-glafs.

When the prifms are placed in the manner reprefented in fig. 3. the point e of the image we is illuminated only by the rays which fall on the object-glafs between A and F, and the point 2w only by the rays which fall on the object-glafs between B and G. Now the angles CRF, CRG, equal to the refractions of the prifms, being conftant, the fpaces FC, CG, will increafe in proportion as the diftances RF, RG, increafe, and the fpaces AF, GB, diminifh as much; and therefore, the images at the point of mutual contact e2w will be each illuminated by half the rays which fall on the object-glafs when the prifms are placed clofe to the object-glafs; but will be enlightened lefs and lefs the nearer the prifms are brought to the focus of the object-glafs.

But

But when the prifms are placed in the manner fhewn in fig. 4. the images at the point of contact, as the prifms are removed from the object-glafs towards the eye-glafs, will be enlightened with more than half the rays that fall on the object-glafs, and will be moft enlightened when the prifms are brought to the focus itfelf; for the point 2*e* of the image 2*w*2*e* will be enlightened by all the rays EE that fall on the object-glafs between B and F, and the point *w* of the image *we* will be enlightened by all the rays ww which fall on the object-glafs between A and G. But the difference of the illuminations is not very confiderable in achromatic telefcopes, on account of the great aperture of the object-glafs; as the greateft fpace FG is to the focal diftance of the object-glafs, as the fum of the fines of the refractions of the prifms is to the radius.

There is a third way, and perhaps the beft, of placing the prifms, fo as to touch one another along their fides which are at right angles to the common fections of their refracting planes. In this difpofition of the prifms, the images will be equally enlightened, namely, each with half the rays which fall on the object-glafs, wherever the prifms be placed between the object-glafs and eye-glafs.

From

From what has been shewn it appears, that this instrument, which may be properly called the prismatic micrometer, will measure any angle that does not exceed the sum of the refractions of the prisms, excepting only very small angles, which cannot be taken with it on account of the vanishing of the pencils of rays at the juncture of the two prisms near the focus of the object-glass; that it will afford a very large scale, namely, the whole focal length of the object-glass for the greatest angle measured by it; and that it will never be out of adjustment; as the point of the scale where the measurement begins (or the point of o) answers to the focus of the object-glass, which is a fixed point for celestial objects, and a point very easily found for terrestrial objects. All that will be necessary to be done, in order to find the value of the scale of this micrometer, will be to measure accurately the distance of the prisms from the focus when the instrument is set to measure the apparent diameter of any object subtending a known angle at the centre of the object-glass, which may be easily found by experiment, as by measuring a base and the diameter of the object observed placed at the end of it, in the manner practiced with other micrometers: for the angle subtended by this object will be to the angle subtended by a celestial object, or very remote land object, when the

diftance of the prifms from the principal focus is the fame as it was found from the actual focus in the terreftrial experiment, as the principal focal diftance of the object-glafs is to the actual focal diftance in the faid experiment.

It will, I apprehend, be the beft way in practice, inftead of one prifm to ufe two prifms, refracting contrary ways, and fo divide the refraction between them (as reprefented in fig. 3. and 4.). Achromatic prifms, each compofed of two prifms of flint and crown-glafs, placed with their refracting angles contrary ways, will undoubtedly be neceffary for meafuring angles with great precifion by this inftrument: and I can add with pleafure, that I find by experiment made with this inftrument, as it was executed by Mr. DOLLOND with achromatic prifms, ground with great care for this trial above a twelve-month ago, that the images after refraction through the prifms appear very diftinct; and that obfervations of the apparent diameters of objects may be taken in the manner here propofed with eafe and precifion.

Two or more fets of prifms may be adapted to the fame telefcope, to be ufed each in their turn, for the more commodious meafurement of different angles. Thus it may be very convenient to ufe one fet of prifms for mea-

furing angles not exceeding 36', and confequently fit for meafuring the diameters of the Sun and Moon, and the lucid parts and diftances of the cufps in their eclipfes; and another fet of prifms to meafure angles not much exceeding one minute, and confequently fit for meafuring the diameters of all the other planets. This latter fet of prifms will be the more convenient for meafuring fmall angles, on account of a fmall imperfection attending the ufe of this micrometer, as before mentioned; namely, that angles cannot be meafured with it when the prifms approach very near the focus of the object-glafs, the pencils of rays being there loft at the point where the prifms touch one another.

Upon the principles that have been here explained, a prifm placed within the telefcope of an aftronomical inftrument, adjufted by a plumb-line or level, to receive all the rays that pafs through the object-glafs, may conveniently ferve the purpofe of a micrometer, and fuperfede the ufe both of the vernier fcale and the external micrometer; and the inftrument may then be always fet to fome even divifion before the obfervation. Thus the ufe of a telefcopic level may be extended to meafure with great accuracy the horizontal refractions, the depreffion of the horizon of the fea, and fmall altitudes and depreffions of land objects. Time and experience

will

will doubtlefs fuggeft many other ufeful applications of this inftrument.

A paper from the learned Abbé BOSCOVICH was read before this Society the ninth of laft June, defcribing a fimilar contrivance as an invention of the Abbé ROCHON, in which the Abbé BOSCOVICH himfelf alfo claims fome fhare; I therefore defire to acquaint this Society, that I communicated this invention to Mr. DOLLOND, and had it executed by him; and alfo fhewed the inftrument itfelf, fo executed, to my efteemed friend ALEXANDER AUBERT, efq. fellow of this Society, a gentleman very well quali-fied to judge of things of this nature, above a twelve-month before the communication of the Abbé BOSCO-VICH's paper, as will appear from their written attefta-tions, drawn up at my defire, defcribing the particulars of the communication of this invention which I made to them fo long ago.　May I be permitted to remark, that this inftrument having been executed by my directions, in feveral forms, by Mr. DOLLOND, between the months of March and Auguft, 1776, and fet up and tried at his houfe in the prefence of feveral of his workmen, could not be confidered as an abfolute fecret concealed from the public.　However, I doubt not that the following attefta-tions of Mr. AUBERT and Mr. DOLLOND will fufficiently prove my title to this invention of the prifmatic micro-

meter;

meter; and I take this opportunity of exhibiting to the Society the inftrument itfelf, mentioned in Mr. DOLLOND's letter as executed by himfelf according to my directions, and fent to the Royal Obfervatory in the month of Auguft 1776.

Greenwich,
December 11, 1777.

—————————

TO THE REV. DR. MASKELYNE.

REV. SIR,

St. Paul's Church-yard,
Nov. 22, 1777.

ACCORDING to your defire I fend the following particulars of the experiments which were made by your directions, for compleating a new kind of micrometer for meafuring fmall angles. About the beginning of April 1776, I received your firft directions refpecting this matter, which were to make two prifmatic glaffes or wedges of fuch angles that rays of light, which paffed through them, fhould be refracted about 18′ of a degree: thefe were to be placed between the object-glafs and eye-glafs of an achromatic telefcope about 30 inches long. The angular edges of the two prifmatic glaffes were to be placed in contact with each other; they were to be moved in a parallel pofition from the object-glafs to the

focus

focus of the eye-glafs, and to be of fuch a fize as to cover the aperture of the object-glafs when brought clofe to it. By the refraction of thefe wedges two images were formed in the telefcope, which were at the greateft diftance (about 36′) when the wedges were clofe to the object-glafs, and approached as they were moved towards its focus, where they united; fo that the whole focal diftance of the object-glafs was to be the length of the fcale for meafuring the angular diftance of the two images formed in the telefcope. When thefe wedges were applied, as above defcribed, the two images were found to be coloured to a great degree, occafioned by the refraction of the wedges. This defect you directed me to remove by making the prifmatic glaffes or wedges achromatic, on the fame principles as the achromatic object-glaffes; and, after fome difficulties, this was effected; the two images formed in the telefcope appeared free from colours and diftinct. The above experiments were made in a rough wooden tube, with an inconvenient method of moving the wedges by hand: in this ftate it was when fhewn to ALEXANDER AUBERT, efq. F. R. S. towards the end of May, 1776; after which you defired to have it done in a more compleat manner, in a brafs tube, with a means of turning the tube round to take angles in different directions, and a method of moving

moving the wedges with a fcrew. This was compleated about the middle of Auguft in the fame year, and then fent to the Royal Obfervatory. I have the honour to be,

REVEREND SIR,

Your obedient humble fervant,

PETER DOLLOND.

I HEREBY certify, that in the month of May, 1776, the Rev. Mr. MASKELYNE, Aftronomer-royal, produced to me, at Mr. DOLLOND's houfe in St. Paul's church-yard, and in his prefence, as a new invention of his own, an inftrument for meafuring fmall angles, confifting of two achromatic prifms or wedges applied between the object-glafs and eye-glafs of an achromatic telefcope about 30 inches long, by moving of which wedges nearer to, or farther from, the object-glafs, the two images of an object produced by them appeared to approach to, or recede from, each other, fo that the focal length of the object-glafs became a fcale for meafuring the angular diftance of the two images.

ALEX^R. AUBERT.

XXXVII. *The Report of the Committee appointed by the Royal Society to confider of the beft Method of adjufting the fixed Points of Thermometers; and of the precautions neceffary to be ufed in making Experiments with thofe Inftruments.*

Read June 19, and Dec. 28, 1777.

IT is univerfally agreed by all thofe who make and ufe FAHRENHEIT's thermometers, that the freezing point, or that point which the thermometer ftands at when furrounded by ice or fnow beginning to melt, is to be called 32°; and that the heat of boiling water is to be called 212°: but for want of further regulations concerning the manner in which this laft point is to be adjufted, it is placed not lefs than two or three degrees higher on fome thermometers, even of thofe made by our beft artifts, than on others. The two principal caufes of this difference are, firft, that it has never been fettled at what height of the barometer this point is to be adjufted [a];

and

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[a] FAHRENHEIT found that the heat of boiling water differed according to the height of the barometer; but fuppofed the difference to be much greater than

and fecondly, that fo much of the quickfilver in the thermometer as is contained in the tube, is more heated in the method ufed by fome perfons, than in that ufed by others. To fhew that this laft circumftance ought by no means to be difregarded, fuppofe that the ball of a thermometer be dipped into boiling water as far as to the freezing point, and confequently that the length of the column of quickfilver in that part of the tube which is not immerfed in the water be 180°; and fuppofe that the heat of that part of the column of quickfilver be no more than 112°. If the thermometer be now intirely immerfed in the water, the heat of this column will be increafed 100°; and confequently its length will be increafed by $\frac{100}{11500}$ parts of the whole, as quickfilver expands $\frac{1}{11500}$ part of its bulk by each degree of heat; and confequently the thermometer will ftand $\frac{180 \times 100}{11500}$ or rather more than $1°\frac{1}{2}$ higher than it did before.

Another thing to be confidered in adjufting the boiling point is, that if the ball be immerfed deep in the water, it will be furrounded by water which will be com-

than it really is. Mr. DE LUC has fince, by a great number of experiments made at very different heights above the level of the fea, found a rule by which the difference in the boiling point, anfwering to different heights of the barometer, is determined with great exactnefs. According to this rule the alteration of the boiling point by the variation of the barometer from $29\frac{1}{2}$ to $30\frac{1}{2}$ inches is $1°.59$ of FAHRENHEIT.

preffed by more than the weight of the atmofphere, and on that account will be rather hotter than it ought to be.

We are of opinion, that the quickfilver in the tube ought, if poffible, to be kept of the fame heat as that in the ball, and that the ball ought not to be immerfed deep in the water. Thefe two requifites may be obtained by ufing a veffel covered fo as to allow no more paffage than what is fufficient for carrying off the fteam; for then, if the thermometer be inclofed in this veffel in fuch manner that the boiling point fhall rife but a little way above the cover, almoft all the quickfilver in the tube will be furrounded by the fteam of the boiling water, and confequently will be nearly of the fame heat as the water itfelf: we therefore made fome experiments to determine how regular the boiling point would be when tried in fuch veffels, both when the ball was immerfed in the water, and when it was expofed only to the fteam as recommended by Mr. CAVENDISH [b]

The veffel ufed in thefe experiments is reprefented in fig. 1. AB*ba* is the pot containing the boiling water; D*d* is the cover; E is a chimney for carrying off the fteam; M*m* is the thermometer faftened to a brafs frame; this thermometer is paffed through a hole F*f* in the cover, and refts thereon by a circular brafs plate G*g* faftened

[b] Phil. Tranf. vol. LXVI. p. 380.

to its frame, a piece of woollen cloth being placed between G*g* and the cover, the better to prevent the escape of the vapours.

There were two pots of this kind used by us; one five inches in diameter and nine deep; the other, $4\frac{1}{4}$ in diameter and 23 deep. Two of the thermometers principally used were the short ones, the brass plate (G*g*) being placed only $3\frac{3}{4}$ inches above the top of the ball, and the boiling point rising not much above that plate: the third thermometer was much longer, the plate (G*g*) being 17 inches above the ball. They were all three quick; the first containing only $2\frac{1}{2}$ degrees to an inch; the second $5°$; and the third $10°$. The first had a cylinder instead of a ball $1\frac{1}{2}$ inch long and $\frac{4}{10}$ in diameter[c]; the two others had spherical balls, about $\frac{1}{4}$ of an inch in diameter.

On trying these thermometers in the above mentioned vessels, with the water rising two or three inches above the top of the ball, we found some variations in the height according to the different manner of making the experiment, but not very considerable; for the most part there was very little difference whether the water boiled

(c) In the two short thermometers the quicksilver would have descended into the ball when cold, had not the tube been swelled a little, close to the ball, in order to prevent it.

fast

faſt or very gently; and what difference there was, was not always the ſame way, as the thermometer ſometimes ſtood higher when the water boiled faſt, and ſometimes lower. The difference, however, ſeldom amounted to more than $\frac{1}{10}$th of a degree, unleſs a conſiderable part of the ſides of the pot were expoſed to the fire; but in ſome trials which we made with the ſhort thermometers in the ſhort pot, with near four inches of the ſide of the veſſel expoſed to the fire[d], they conſtantly ſtood lower when the water boiled faſt than when ſlow, and the height was in general greater than when only the bottom of the pot was expoſed to the fire. This difference however was not perceived in the trials of the long thermometer in the deep pot, as there ſeemed very little difference in the height whether the water boiled faſt or ſlow, or whether more or leſs of the ſide of the pot was expoſed to the fire. The greateſt difference obſerved in the ſame thermometer, on the ſame day and in the ſame water, according to the different manner of trying the experiment, was half a degree.

(d) In all our experiments, the water was boiled over a portable black-lead furnace, covered with an iron plate, which had a hole cut in it juſt big enough to receive the bottom of the pot; ſo that, by paſſing the bottom through this hole to a greater or leſs depth, we could expoſe more or leſs of the ſides to the fire. In the other experiments, not more than one inch of the ſides was ever expoſed to the fire.

We

We made some trials with the long thermometer in the deep pot, to determine how much the height of the boiling point was affected by a greater or less depth of water above the ball. By a mean of the experiments it stood ,66 of a degree higher when the water rose 15 inches above the ball, than when it was only three inches above the ball; so that increasing the depth of water above the ball by 11 inches, raised the thermometer ,66 of a degree, that is ,06 for each inch.

We would by no means infer however from hence, that it is a constant rule, that the height of the boiling point is increased ,06 of a degree by the addition of each inch in the depth of the water above the ball; as perhaps the proportion would be found very different in greater depths of water or in wider vessels.

If this rule is constant, it would shew that, when the pressure on that part of the water which surrounds the ball is increased by increasing the depth of water above the ball, the height of the boiling point is not altered thereby more than half as much as by an equal increase of pressure produced by an alteration in the weight of the atmosphere: for the pressure on that part of the water which surrounds the ball is as much increased by an alteration of 11 inches in the depth of the water above the ball, as by an increase of $\frac{11}{13\frac{1}{2}}$ of an inch in the height

of

of the barometer; and fuch an alteration in the height of the barometer is fufficient to raife the boiling point $1°,3$.

It feems as if the height of the boiling point was in fome meafure increafed by having a great depth of water below the ball, as in general the fhort thermometers flood higher when tried in the deep pot than in the fhort one; this effect, however, did not always take place. In the former of thefe cafes, the depth of water below the ball was about 18 inches, in the other only 4; but the depth of water above the ball was the fame in both cafes.

It muft be obferved, that when there was a great depth of water in the veffel, either above or below the ball, the experiments were much more irregular, and the quickfilver in the tube remained much lefs fteady than when it was fmall. When the depth of water in the veffel is great, it is apt to boil in gufts, which feems to be the caufe of this irregularity; though we could not perceive any regular connection between thefe gufts and the rifing of the thermometer.

In the experiments made with the water not rifing fo high as the ball, fo that the thermometer was expofed only to the fteam, we very feldom found any fenfible difference whether the water boiled faft or flow: but

whenever

whenever there was any, the greater height was when the water boiled faft; the difference, however, never amounted to more than $\frac{1}{20}$th of a degree.

There was fcarce ever any fenfible difference whether the fhort thermometers were tried in the fhort pot or the deep one, though in the former cafe the ball was raifed very little above the furface of the water, and in the latter not lefs than 14 inches: neither did we find any fenfible difference in trying them in the tall pot, whether there was a greater or lefs depth of water in the veffel.

As it was neverthelefs fufpected, that the heat of the fteam might poffibly be lefs near the top of the pot than lower down (for in thefe experiments the ball of the thermometer was always at the fame depth below the cover, though its height above the furface of the water was very different) we made two holes in the fide of a pot four inches deeper than the deepeft of the foregoing, one near the top of the pot, and the other not far from the bottom, and paffed the ball of the thermometer through one or the other of thefe holes, taking care to ftop up both holes very carefully, fo that no air could enter into the pot by them: no fenfible difference could be perceived in the height, whether the thermometer was placed in the upper or lower hole, though in one

cafe

cafe the ball was only three inches, and in the other 21 inches, below the cover.

The heat of the fteam therefore appears to be not fenfibly different in different parts of the fame pot; neither does there appear to be any fenfible difference in its heat, whether the water boil faft or flow; whether there be a greater or lefs depth of water in the pot; or whether there be a greater or lefs diftance between the furface of the water and the top of the pot; fo that the height of a thermometer tried in fteam, in veffels properly clofed, feems to be fcarce fenfibly affected by the different manner of trying the experiment.

Though, as was before faid, there was fcarce any difference in the height of the quickfilver, whether the water boiled faft or flow, yet, when the water boiled flow, the thermometer was a great while before it rofe to its proper height; and when it boiled very flow, it feemed doubtful whether it would have ever rifen to it, efpecially if the ball was raifed a great way above the furface of the water: but when, by making the water boil brifkly, the thermometer had once rifen to its proper height, the water might then be fuffered to boil very gently, even for a great length of time, without the thermometer finking fenfibly lower[e].

All

[e] The reafon of this feems to be that, while any air is left in the pot, the

All three thermometers were found to ftand, in gene-ral, from 30 to 65 hundredths of a degree higher when the ball was immerfed a little way in the water (neglect-ing thofe obfervations in which much of the fides of the pot were expofed to the fire) than when it was tried in fteam: at a medium they ftood $\frac{48}{100}$ higher, which is equal to the difference produced by a variation of $\frac{3}{10}$ths of an inch in the barometer; fo that the boiling point, adjufted at a given height of the barometer, with the ball im-merfed a little way in the water, will in general agree with that adjufted in fteam, when the barometer is $\frac{3}{10}$ths of an inch higher.

It muft be obferved, that in all thefe experiments a piece of flat tin plate was laid loofely on the mouth of the chimney E, fo as to leave no more paffage for the fteam than what was fufficient to prevent the tin plate from being lifted up. In trying the thermometers in fteam, this is by no means unneceffary; for, if the cover of the pot does not fit pretty clofe, the thermometers will immediately fink feveral degrees on removing the tin plate; but, when their balls are immerfed in the water, the removal of the tin plate has no fenfible effect.

fteam cannot acquire its full degree of heat; and that when the water boils very gently, the air is not eafily intirely expelled from the pot. That the fteam will not acquire its full degree of heat while any air is left in the pot will appear from the next paragraph but one.

If this cover to the chimney had been heavy, the included fteam might have been fo much compreffed thereby, that the water and fteam might have acquired a confiderably greater heat than they ought to have done; but as this plate lay loofe on the chimney, and as its weight was not greater than that of a column of quickfilver, whofe bafe is equal to that of the mouth of the chimney, and whofe altitude is $\frac{1}{50}$th of an inch, the excefs of the compreffion of the included fteam above that which it would fuffer in an open veffel, could not be greater than that which would be caufed by an increafe of $\frac{1}{50}$th of an inch in the height of the barometer, which is too fmall to be worth taking notice of; for, if the excefs of compreffion was greater than that, the tin plate muft neceffarily be lifted up fo much as to afford a fufficient paffage for the fteam to efcape faft enough, though urged by no greater force than that.

Though in the different trials of the fame thermometer in fteam, on the fame day, and with the fame water, fo little difference was obferved, according to the different manner of trying the experiment; yet there was a very fenfible difference between the trials made on different days, even when reduced to the fame height of the barometer, though the obfervations were always made either with rain or diftilled water. The difference,

4   however,

however, never amounted to more than a quarter of a degree, except in one thermometer, in which there were three obfervations out of eighteen which differed more than that; one of them differed fo much as $0.65°$ from fome of the reft. In the obfervations made with the ball immerfed a little way in the water, there was a greater difference between the obfervations of different days, even neglecting thofe in which much of the fides of the pot were expofed to the fire. In two of the thermometers the different obfervations differed about $\frac{15}{100}$ of a degree from each other; but in the other thermometer they varied $\frac{8}{10}$ths.

We do not at all know what this difference could be owing to, efpecially in the obfervations in fteam. It could not proceed intirely from fome unknown difference in the water; for, if it did, the difference between the different thermometers fhould have been always the fame, which was not the cafe, though in general, on thofe days in which one thermometer ftood high, the others did alfo, efpecially in the trials in fteam. Moreover, as far as can be perceived from our experiments, there feems to be very little difference between different waters with refpect to the heat which they acquire in boiling. We could not be fure that there was any difference between rain or diftilled water and pump water, provided

the

the latter had boiled long: neither did any difference feem to arife from the water containing fuch fubftances as are difpofed to part readily with their phlogifton; for, on trying the thermometers in the fteam of diftilled water, their height was not fenfibly altered by pouring in a fmall quantity of a folution of liver of fulphur, or of iron filings imperfectly rufted. The thermometer, how-ever, feemed to ftand fenfibly lower in pump water be-ginning to boil, than in the fame water long boiled, but the difference fcarcely exceeded $\frac{1}{10}$th or $\frac{1}{5}$th of a degree.

We made fome experiments to determine the heat of water boiling in open veffels. In general, when the veffel was almoft full, and the water boiled faft, and the ball of the thermometer was held from three-quarters to two or three inches under water, and alfo in that part of the veffel where the current of water afcended up-wards, that is, in the hotteft part of the water, its heat was not much different from that of the fteam of water boiling in clofed veffels, varying only from a quarter of a degree more than that, to as much lefs; but if the water boiled gently, its heat would frequently be half or three-quarters of a degree cooler than the fteam. If the ex-periment was tried in the deep pot with fuch a quantity of water in it that the furface was at leaft 14 or 15 inches below the top of the pot, fo that though the veffel was

open,

open, yet the water was not much expofed to the air, its heat then feemed fcarcely lefs than when boiled in clofed veffels.

In making thefe experiments we chiefly made ufe of the two fhort thermometers, in which, as the quantity of quickfilver contained in the tube was fmall, the error arifing from that part of the quickfilver being not heated equally with that in the ball, could be but fmall: for example, in the fecond of the fhort thermometers, the number of degrees contained in that part of the tube between the circular plate Gg and the ball was 18°. In the experiments in fteam this part of the tube was heated to the fame degree as the ball. Suppofe now, that in open veffels it was heated only to 122°, or was 90° cooler than the ball, it is plain, that the thermometer would ftand only $\frac{18 \times 90}{11500}$, or $\frac{1}{7}$th of a degree lower than it did in fteam, provided the heat of the quickfilver in the ball was the fame in both cafes. In the other fhort thermometer, as there were only half as many degrees to an inch, the error was only half as great.

In feveral of the experiments, however, we made ufe of the long thermometer; but then it was neceffary to make an allowance on account of the quickfilver in the tube being not heated equally with that in the ball. The

better

better to enable us to do this, we made use of a thermo-
meter tube, filled with quickfilver in the same manner
as a thermometer, only without any ball to it, or a ther-
mometer without a ball, as we may call it. A small brass
plate was fixed to the tube near the top of the column
of quickfilver, to shew the heat as in a common ther-
mometer.   In all our experiments with the long ther-
mometer in open vessels, this tube without a ball, was
placed by its side; whence, as the quickfilver in the tube
of the long thermometer could hardly fail of being
nearly of the same heat as that in the tube without a
ball, we knew pretty nearly the heat of the quickfilver
in the tube of the former, and consequently how much
higher it would have stood if the quickfilver in its tube
had been of the same heat as that in the ball.   For exam-
ple, on October 19, the long thermometer tried in an
open vessel, the water boiling fast, stood 1°.65 lower than
it did when tried in steam the same day, the quickfilver
in the tube without a ball standing at the same time at
109°: we may therefore conclude, that the heat of the
quickfilver, in that part of the tube of the long thermo-
meter which was not immersed in the water, was also
109°; and consequently, as that part of the tube con-
tained about 170°, the thermometer stood $\frac{170 \times 103}{11500}$, or

1°52

1°.52 lower than it would have done if the quickfilver in the tube had been of the fame heat as that in the ball; and, confequently, the quickfilver in the ball of the thermometer was in reality .07 cooler than when tried in fteam.

We examined the boiling points of feveral thermometers, made by different artifts, by trying them in fteam when the barometer was at 30.1, and finding what divifion on the fcale the quickfilver ftood at. The difference of the extremes was $3°\frac{1}{4}$; but, by a mean of all, it was found to ftand at 213°.1, and confequently would have ftood at 212°, if the barometer had been at 29.4; fo that if the boiling point was to be adjufted, either in fteam, when the barometer is at 29.4, or with the ball immerfed two or three inches in water, when the barometer is at 29.1, it would agree beft with the mean of the abovementioned thermometers. But as it feems to be of no great fignification to make the boiling point agree very nearly with the mean of the thermometers made at prefent, when the extremes differ fo widely; and as we apprehend that it will be more convenient to the makers that fome height fhould be chofen which differs lefs from the mean, as thereby they will more frequently have an opportunity of adjufting the boiling point without the trouble and danger of miftakes which attend the

making

making a correction, we recommend, that the boiling point should be adjusted when the barometer is at 29.8, if the person chufes to do it in steam; or when the barometer is at 29½, if he choofes to do it in clofe veffels, with the ball immerfed to a fmall depth under the water. Our reafon for pitching upon this precife height is, that thereby the boiling point will differ from Mr. DE LUC's boiling point, by a fimple fraction of the degrees of his common fcale, namely three-quarters of a degree higher.

We are informed by Mr. DE LUC, that the method he ufed in adjufting the boiling point, though he forgot to mention it in the *Récherches fur les Modifications de l'Atmofphere*, was to wrap rags round the tube of the thermometer, and to try it with the ball immerfed in water in an open veffel, of the form defcribed in the abovementioned book, while boiling water was poured at different times on the rags, in order that the quickfilver in the tube might be heated, if poffible, to the fame degree as that in the ball. As well as we can judge from the abovementioned experiments in open veffels, and from the few trials we have made of this method, we are inclined to think, that the boiling point adjufted this way will in general differ but little from that adjufted in fteam at the fame height of the barometer, efpecially if the thermometer be not very long, and do not extend a great

way

way below the freezing point[f]; consequently, as Mr. DE LUC's boiling point was adjusted when the barometer was at 27 Paris or 28.75 English inches, it will stand lower than that adjusted in the manner recommended by us, by three-quarters of a degree of his scale; or $80\frac{1}{4}$ on DE LUC's thermometer, will answer to 212° on FAHRENHEIT's adjusted in the manner proposed.

Though the boiling point be placed so much higher on some of the thermometers now made than on others, yet we would not have the reader think that this can make any considerable error in the observations of the weather, at least in this climate; for an error of $1°\frac{1}{2}$ in the position of the boiling point will make an error of only half a degree in the position of 92°, and of not more than a quarter of a degree in the point of 62°. It is only in nice experiments, or in trying the heat of hot liquors, that this error in the boiling point can be of much signification.

(f) In order to see how much the quicksilver in the tube of the thermometer would be heated in this method of adjusting the boiling point, we took the abovementioned tube without a ball, wrapped it round with rags, and poured boiling water on it as above described: the heat of the quicksilver therein was found to be about 21° less than that of boiling water; and, therefore, the boiling point of a thermometer, adjusted in this manner, supposing the thermometer to be dipped into the water as far as to the point of 32°, should stand about one-third of a degree lower than it would do if the quicksilver in the tube was heated equally with that in the ball.

There is another circumſtance that we have not yet taken notice of, which, in ſtrictneſs, cauſes ſome error in thermometers, namely, the difference of expanſion of the glaſs tube and the ſcale. But this error is in almoſt all caſes ſo ſmall as to be not worth regarding; we have, however, in the note below given a rule for computing the value of it *(g)*.

(g) The uſual way of adjuſting thermometers is, to mark the boiling and freezing points on the glaſs tube, and not to ſet off thoſe points on the ſcale till ſome time after, when the tube and ſcale may both be ſuppoſed to be nearly of the temper of the air in the room; conſequently, when the thermometer is expoſed to a greater heat than that, the ſcale, if of braſs, will expand more than the glaſs tube, and the diviſions on it will be longer than they ought to be; but, if the ſcale be of wood, it will expand leſs than the glaſs tube, and the diviſions will be too ſhort. Let now the heat of the air, when the diviſions were ſet off on the ſcale, be called A; let the degree of heat which the thermometer ſtands at in the experiment be called D; and let the degree anſwering to that point of the ſcale in which the thermometer is faſtened to the ſcale be called F. Then, if all parts of the thermometer and ſcale are heated equally, and the ſcale is of braſs, the thermometer will appear to ſtand lower than it ought to do by the $\dfrac{D-F \times D-A}{165000}$ part of a degree, obſerving, that if $D-F \times D-A$ is negative, it will ſtand higher than it ought to do; but if the ſcale is of wood, it will ſtand higher than it ought to do by the $\dfrac{D-F \times D-A}{216000}$ part of a degree. If the thermometer be faſtened to the ſcale by the ball, or any part of the tube lower than the obſerved heat, the error will be the ſame, whether that part of the tube and ſcale, which is above the obſerved degree, be of the ſame heat as the ball or not: but if the thermometer is faſtened to the ſcale by the top of the tube, as is frequently done, then the error will vaniſh whenever that part of the tube and ſcale, which is above the obſerved degree, is not much heated. This rule is founded on Mr. SMEATON's experiments, who found, that white glaſs expands $\frac{1}{100}$th of an inch in a foot by 180° of heat; that braſs wire expands $\frac{3}{2320}$; and that wood expands ſcarce ſenſibly.

In making experiments with thermometers, it evidently is equally neceffary that the quickfilver in the tube fhould be of the fame heat as that in the ball, as it is in adjufting the boiling point: for this reafon, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if poffible, to be immerfed as far as to the top of the column of quickfilver in the tube. As this, however, would often be very difficult to execute, the obferver will frequently be obliged to content himfelf with immerfing it to a much lefs depth. But then as the quickfilver, in a great part of the tube, will be of a different heat from that in the ball, it will be neceffary, where any degree of accuracy is required, to make a correction, on that account, to the heat fhewn by the thermometer. If the heat of the quickfilver in the tube be known, the correction may readily be made by help of the annexed table; the only difficulty lies in eftimating what that heat may be. In all probability the heat of the quickfilver in the tube will not be very different from that of the air which furrounds it[h]; but as

that

(h) This muft evidently be the cafe, unlefs the quickfilver in the tube is confiderably heated by its contact with that in the ball. To fee whether this was the cafe, fome fand was heated in a fmall copper difh over a lamp to the heat of about 212°, and the abovementioned tube, without a ball, laid horizontal with the end extending about half an inch over the fand; but, to prevent its being heated thereby, a piece of wood, about a quarter of an inch thick,

was

that air will be affected by the fteam of the liquor, and the fire by which it is heated, it will commonly be of a very different heat from the reft of the air of the room in which the experiment is made; but as no great nicety is required in eftimating the heat of the quickfilver in the tube, infomuch that a miftake of 25° therein will caufe an error of only half a degree in the correction, when the number of degrees in that part of the tube which is not immerfed in the liquor is not more than 220°, it will commonly be not difficult to guefs at the heat of the quickfilver in the tube as near as is required[(i)]. But if the obferver is defirous of more accuracy,

was laid between the fand and it. After it had remained a fufficient time in this fituation, the divifion which the quickfilver ftood at was obferved. The piece of wood was then removed, and the end of the tube laid in the fand, which was heaped over it fo that about half an inch of the column of quickfilver was intirely furrounded by the hot fand, and muft therefore be heated to nearly the fame degree as it. The quickfilver in the tube rofe very little higher than before, and feemingly not more than might be owing to the expanfion of the half inch of quickfilver which was furrounded by the fand; fo that it fhould feem, that heating one end of the column of quickfilver does not communicate much heat to the reft of the column; and confequently, that, when the ball of a thermometer is immerfed in hot liquor, the quickfilver in the tube will not be much hotter than the furrounding air.

(i) The better to enable the reader to guefs at the heat of the quickfilver in the tube, in cafes of this kind, we tried how much the quickfilver in the above-mentioned tube, without a ball, would be heated when held over a veffel of boiling water. It is true, that thefe experiments cannot be of any great fervice towards this purpofe, as the tubes will be very differently heated, according to

the

racy, he may find the heat of the furrounding air by holding the ball of a fmall thermometer near the tube of

the degree of heat of the fluid, and the quantity of fteam which it furnifhes, and according to the nature of the fire by which it is heated; yet as the experiments may perhaps ferve in fome meafure to rectify our ideas on this head, we will give the refult. When the abovementioned tube without a ball *, the length of the column of quickfilver in which was 15 inches, was held perpendicularly over the veffel of boiling water, with its bottom even with the furface of the water, the heat of the quickfilver was in all the trials we made from 68 to 28° hotter than the air of the room. If the tube was held inclined to the horizon, in an angle of about 30°, with the bottom of the column of quickfilver reaching not more than three quarters of an inch within the circumference of the pot, fo that the column of quickfilver was as little heated by the fteam as could eafily be done, it was from 30 to 7° hotter than the air. When a fhorter tube of the fame kind, in which the column of quickfilver was feven inches, was ufed, the quickfilver was from 62 to 44° hotter than the air, when held perpendicularly, and from 49 to 36° hotter when held inclined. The water in thefe trials frequently boiled pretty faft, but never very violently. It was in general heated over a portable black lead furnace placed in the middle of the room; but it was once heated over an ordinary chafing-difh, when the quickfilver in the long tube, held perpendicularly, was found to be 64° hotter than the air, When the experiments were tried without doors, the heat of the quickfilver in the tube would vary very much, according as the wind blew the fteam and hot air from or towards the tube, but it fometimes rofe as high as it did within doors.

The moft convenient method we know of making thefe tubes without a ball is, to fill a thermometer in the ufual manner, and heat the ball till there is a proper quantity of quickfilver in the tube, and then to make the column of quickfilver feparate at the neck of the ball, and run to the extremity of the tube, fo as to leave a vacuum between the ball and the column of quickfilver, as is expreffed in fig. 2. where the fhaded part AD reprefents the column of quickfilver, and BA that part in which there is a vacuum. The tube muft then be fealed fome-

where

of the thermometer with which he tries the heat of the liquor; or, what will be much better, he may have a tube without a ball, fuch as is above defcribed, faftened to the frame of the thermometer, on one fide of the tube; or if he has two fuch tubes, of different lengths, it will be ftill more accurate.

To avoid the inconvenience of this correction, per-haps it may be thought, that both in adjufting the boiling point and in trying the heat of liquors, it would be better that not much more than the ball of the thermometer fhould be immerfed, and that the tube fhould be held in-clined in fuch manner as to be heated as little as poffible; as it may be faid, that by this means you will find the heat of liquors pretty nearly, without the trouble of making any correction; and that, though in ftrictnefs a correction would be required in obferving the heat of the air with

where between B and A as at E, and cut off there; after which it muft be held with the end D upwards, fo as to make the column of quickfilver run to the extremity E: by this method of filling it is plain, that no fenfible quantity of air can be left between E and the column of quickfilver; but yet the quickfilver will be apt not to run fufficiently clofe to the extremity E, as the weight of the column will be fcarcely fufficient to force it into the narrow fpace which will commonly be left in fealing the tube, efpecially when held nearly horizontal: for this reafon it will be proper to open the tube at D, fo as to let in the air, and then feal it again. It muft be obferved, that the fpace left between D and the column of quickfilver ought not to be lefs than the tenth part of the length of the column of quickfilver, as otherwife the included air might be too much compreffed by the expanfion of the quickfilver when much heated.

fuch

fuch thermometers, yet the heat of the atmofphere never differs fo much from the mean heat, as to make that cor-rection of much confequence [k]. But, on the other hand, this method of making and ufing thermometers is much lefs exact than the former, and therefore is unfit for nice experiments; and, befides, a correction would be as ne-ceffary with this kind of thermometer in trying the heat of air, artificially heated, or in finding the heat of large quantities of hot liquors, in which it would be difficult to prevent the quickfilver in the tube from being heated

(k) The degrees on all thermometers are intended to anfwer to equal por-tions of the folid contents of the tube; and, confequently, if the quickfilver in the tube is kept conftantly of the fame heat as that in the ball, the degrees will anfwer to equal increments of bulk of the whole quantity of quickfilver in the thermometer, that is of a given weight of quickfilver. But if only the quick-filver in the ball is heated, and that in the tube is kept always of the fame heat, the degrees will anfwer to equal increments of a given bulk of quickfilver; fo that the fcale of the thermometers will be really different in thefe two methods of proceeding, and in high degrees the difference will be very confiderable: for example, let two thermometers be made, and in the firft of them let care be taken, both in adjufting the fixed points and in trying the heat of liquors, that the quickfilver in the tube fhall be of the fame heat as that in the ball; and in adjufting the fixed points of the fecond, and in trying the heat of liquors with it, let care be taken that the quickfilver in the tube fhall remain always of the fame invariable heat, and let the freezing and boiling points be marked 32 and 212 on both of them: then will the degree of 620 on the firft anfwer to that of 600 on the fecond; that of 406 to 400; that of 302 to 300; and that of 119.7 to 120; that is, a liquor which appears to be of 620° of heat by the firft will appear to be of 600 by the fecond, &c. It appears from hence, that it would be improper to employ the latter method of adjufting and ufing thermometers for ordinary purpofes, and the former for nice experiments.

by

by the steam, as it is in finding the heat of liquors with the other thermometer, whenever the ball is not immersed to a sufficient depth; so that, on the whole, the former method of making and using thermometers seems much the best.

A much better way of avoiding the trouble of making a correction would be to have two sets of divisions made to such thermometers as are intended for trying the heat of liquors; one of which should be used when the tube is immersed almost to the top of the column of quicksilver; and the other, when not much more than the ball is immersed, in which last case the observer should be careful that the tube should be as little heated by the steam of the liquor as conveniently can be. It is difficult to give rules for constructing this second set of divisions, as the heat of the quicksilver in the tube will be very different according to the temper of the air in the room, the quantity and nature of the fluid whose heat is to be tried, the manner in which it is heated, and the other circumstances of the experiment; but, on the whole, we think that, given in the following table, would be as proper as any.

I

Degree

Degree answering to that point of the tube which is two inches above the ball.

	+75	+50	+25	0	—50	—100	—200	—300	—400	—500	
—500										—500	0
—400									—400	—396.5	0
—300								—300	—297.4	—294.8	0
—250								—248.9	—246.7	—244.6	0
—200							—200.	—198.3	—196.5	—194.8	0
—150							—149.3	—148.	—146.7	—145.4	0
—100						—100	—98.9	—97.7	—96.6	—95.5	0
—50					—50	—49.7	—49.	—48.3	—47.6	—46.9	0
0				0	+0.2	+0.3	+0.6	+0.9	+1.2	+1.5	30
+50											30
+100											35
+150	149.5	149.4	149.	148.7	148.4	147.3					75
+200	198.8	198.5	198.3	198.	197.5	197.					85
+250	247.5	247.1	246.8	246.4	245.7						85
+300	295.8	295.3	294.9	294.4	293.4						85
+350	343.7	343.1	342.5	342.							85
+400	391.1	390.4	389.7	389.1							85
+450	438.1	437.3	436.5	435.7							85
+500	484.7	483.8	482.9	482.							85
+600	576.5	575.4	574.3	573.2							85

To make ufe of this table, feek in the uppermoft ho-rizontal line the degree of the thermometer anfwering to that point of the tube which is two inches above the ball; and in the left-hand column feek the degrees of the fecond fet of divifions; the correfponding numbers in the table are the correfponding degrees of the firft fet, or the degrees which they muft be fet oppofite to. The right-hand perpendicular column fhews the heat which the quickfilver in the tube was fuppofed to be of in forming this table.

Though this fecond fet of divifions be far from accu-rate, yet it is at leaft as much fo as a thermometer ad-jufted in the latter method can be; fo that this double fet of divifions poffeffes all the advantages which can be ex-pected from that method of adjufting thermometers, without the inconveniences.

A table

A table for correcting the obferved height of a thermo-
meter, whenever the quickfilver in the tube is not of
the fame heat as that in the ball.

Diff. of Heat	Degrees not immerfed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	7
50	.2	.4	.7	.9	1.1	1.3	1.5	1.7	2.	2.2	2.4	2.6	2.8	3.1	3
100	.4	.9	1.3	1.8	2.2	2.6	3.0	3.5	3.9	4.4	4.8	5.2	5.7	6.1	6
150	.7	1.3	2.0	2.6	3.3	3.8	4.6	5.2	5.9	6.5	7.2	7.9	8.4	9.2	9
200	.9	1.8	2.6	3.5	4.4	5.1	6.1	7.0	7.8	8.7	9.6	10	11	12	1
250	1.1	2.2	3.3	4.4	5.5	6.4	7.6	8.7	9 8	11	12	13	14	15	1
300	1.3	2.6	3.8	5.1	6.4	7.7	9.1	10	12	13	14	16	17	18	2
350	1.5	3.0	4.6	6.1	7.6	9.1	11	12	14	15	17	18	20	21	2
400	1.7	3.5	5.2	7.0	8.7	10	12	14	16	17	19	21	23	24	2
450	2.	3.9	5.9	7.8	9.8	12	14	16	18	20	22	24	25	27	2
500	2.2	4.4	6.5	8.7	11	13	15	17	20	22	24	26	28	31	3
550	2.4	4.8	7.2	9.6	12	14	17	19	22	24	26	29	31	34	3

To make ufe of this table, in the left-hand perpen-
dicular column look for the number of degrees con-
tained in that part of the tube which is not immerfed in
the fluid whofe heat is to be tried, and in the upper ho-
rizontal line feek the fuppofed difference of heat of the
quickfilver in that part of the tube from that in the ball;
the correfponding number in the table is the correction,
which muft be added to the obferved heat when the

5 N 2   quick-

quickfilver in the tube is cooler than that in the ball, and fubtracted when it is warmer: for example, let the obferved heat of the fluid be 47 5°, let the thermometer be immerfed in the fluid as far as to the degree of 25°, or to that part of the tube which fhould be marked 25° if the divifions were continued long enough; then is the number of degrees in that part of the tube which is not immerfed in the fluid 450; and let the heat of the quick-filver in that part of the tube be fuppofed 100°; and confequently, the difference of heat of the quickfilver in that part of the tube from that in the ball 375; then in the left-hand perpendicular column feek the number 450, and in the upper horizontal line the number 375; the correfponding number in the table, or the correction, is 15°, and therefore the true heat of the fluid is 490°.

This correction may be had very eafily without the help of the table, only by multiplying the number of degrees not immerfed in the fluid by the fuppofed difference of heat, dividing the product by 10000, and diminifhing the quotient by one-eighth part of the whole.

In the following pages we have thrown together the practical rules, which we would recommend to be obferved in adjufting the fixed points of thermometers.

Rules to be observed in adjusting the boiling point.

The most accurate way of adjusting the boiling point is, not to dip the thermometer into the water, but to expose it only to the steam, in a vessel closed up in the manner represented in fig. 4. where AB*ba* is the vessel containing the boiling water, D*d* the cover, E a chimney made in the cover intended to carry off the steam, and *mm* the thermometer passed through a hole in the cover. Those who would make use of this method must take care to attend to the following particulars.

1st, The boling point must be adjusted when the barometer is at 29.8 inches; unless the operator is willing to correct the observed point in the manner directed below.

2dly, The ball of the thermometer must be placed at such a depth within the pot, that the boiling point shall rise very little above the cover; for otherwise part of the quicksilver in the tube will not be heated, and therefore the thermometer will not rise to its proper height. The surface of the water in the pot also should be at least one or two inches below the bottom of the ball; as otherwise the water, when boiling fast, might be apt to touch the ball: but it does not signify how much lower than that the surface of the water may be.

3dly,

3dly, Care muſt be taken to ſtop up the hole in the cover through which the tube is inſerted, and to make the cover fit pretty cloſe, ſo that no air ſhall enter into the pot that way, and that not much ſteam may eſcape. A piece of thin flat tin plate muſt alſo be laid on the mouth of the chimney, ſo as to leave no more paſſage than what is ſufficient to carry off the ſteam. The ſize of this plate ſhould be not much more than ſufficient to cover the chimney, that its weight may not be too great; and the mouth of the chimney ſhould be made flat, that the plate may cover it more completely. It muſt be obſerved, that when the tin plate is laid on the mouth of the chimney, it will commonly be lifted up by the force of the ſteam, and will rattle till it has ſlipped aſide ſufficiently to let the ſteam eſcape without lifting it up. In this caſe it is not neceſſary to put the plate back again, unleſs by accident it has ſlipped aſide more than uſual. If the artiſt pleaſes, he may tie each corner of this plate by a ſtring to prongs fixed to the chimney, and ſtanding on a level with the plate, as thereby it will neceſſarily be kept always in its place[l]; but we would by no means recommend having it made with a hinge, as that might

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(l) Fig. 3. is a perſpective view of the chimney and tin plate; ABCD is the plate; E the chimney; F*f*, G*g*, M*m*, and N*n*, the prongs faſtened to the chimney, to which the four corners of the plate are to be tied by the ſtrings AF, BG, CM, and DN; the ends F, G, M, and N, of the prongs muſt be on a level with the plate, and the ſtrings ſhould not be ſtretched tight.

be

be apt to make it stick, in which case the included vapour might be so much compressed as to cause an error. We would also by no means advise lining the tin plate with leather, or any other soft substance, for the sake of making it shut closer, as that also might be apt to make it stick. The chimney also ought not to be made less than half a square inch in area: for though a smaller chimney would be sufficient to carry off the steam, unless the vessel is much larger than what we used; yet the adhesion which is apt to take place between it and the tin plate when wet, might perhaps bear too great a proportion to the power which the included steam has to lift it off, if it was made much less. It is convenient that the chimney be not less than two or three inches long, as thereby the observer will be less incommoded by the steam; but it would be improper to make it much longer, for the longer the chimney is, the greater disposition has the air to enter into the pot between it and the cover.

It is most convenient not to make the cover fit on tight, but to take on and off easily; and to wrap some spun cotton round that part of the cover which enters into the pot, in order to make it shut closer; or, what seems to answer rather better, a ring of woollen cloth may be placed under the cover, so as to lie between the top

of

of the pot and it. Thefe methods of making the cover fhut clofe can be ufed more conveniently when the cover is made to enter within the pot, as in the figure, than when it goes on on the outfide.

There are various eafy ways by which the hole in the cover, through which the tube of the thermometer is paffed, may be ftopped up, and by which the thermometer may be fufpended at the proper height. The hole in the cover may be ftopped up by a cork, which muft firft have a hole bored through it, big enough to receive the tube, and be then cut into two, parallel to the length of the hole. Another method, more convenient in ufe, but not fo eafily made, is reprefented in fig. 6. which exhibits a perfpective view of the apparatus; A *a* is the cover; H the hole through which the thermometer is paffed; B *b* a flat piece of brafs fixed upon the cover; and D *d* E *e* a fliding piece of brafs, made fo as either to cover the hole H, or to leave it uncovered as in the figure, and to be tightened in either pofition by the fcrew s fliding in the flit M *m*; a femi-circular notch being made in the edge B *b*, and alfo in the edge D *d*, to inclofe the tube of the thermometer: pieces of woollen cloth fhould alfo be faftened to the edges B *b* and D *d*, and alfo to the bottom of the fliding piece D *d* E *e*, unlefs that piece and the cover are made fufficiently flat, to prevent the efcape of the fteam. In

order

order to keep the thermometer suspended at the proper height, a clip may be used like that represented in fig. 7. which by the screw s must be made to embrace the tube tightly, and may rest on the cover. That part of the clip which is intended to bear against the tube, had best be lined with woollen cloth, which will make it stick tighter to the tube, and with less danger of breaking it. Another method, which is rather more convenient, when the top of the tube of the thermometer is bent into a right angle, in the manner frequently practised at present for the sake of more conveniently fixing it to the scale, is represented in the same figure; G*g*F*f* is a plate of brass, standing perpendicularly on the cover, and L*l*N *n* a piece of brass, bent at bottom into the form of a loop, with a notch in it, so as to receive the tube of the thermometer, and to suffer the bent part to rest on the bottom of the loop; this piece must slide in a slit K *k*, cut in the plate L*l*N *n*, and be tightened at any height by the screw T.

4thly, It is best making the water boil pretty briskly, as otherwise the thermometer is apt to be a great while before it acquires its full heat, especially if the vessel is very deep. The observer too should wait at least one or two minutes after the thermometer appears to be stationary, before he concludes that it has acquired its full height.

5thly,

5thly, Though, as was faid before, this appears to be the moft accurate way of adjufting the boiling point; yet, if the operator was to fuffer the air to have any accefs to the infide of the veffel, he would be liable to a very great error: for this reafon we ftrongly recommend it to all thofe who ufe this method, not to deviate at all from the rules laid down without affuring themfelves, by repeated trials with a pretty fenfible thermometer, that fuch alteration may be ufed with fafety. But the covering the chimney with the tin plate ought by no means to be omitted; for though, if the cover of the pot fits clofe, it feldom figni-fies whether the plate is laid on or not, yet, if by acci-dent the cover was not to fit clofe, the omitting the tin plate would make a very great error. Making the chim-ney very narrow would not anfwer the end properly; for, if it was made fo fmall as to make the veffel fuffici-ently clofe when the water boiled gently, it would not leave fufficient paffage for the efcape of the fteam when the water boiled faft.

Another way of adjufting the boiling point is, to try it in a veffel of the fame kind as the former, only with the water rifing a little way, namely from one to three or four inches above the ball, taking care that the boil-ing point fhall rife very little above the cover, as in the former method. In this method there is no need to cover

the

the chimney with the tin plate; and there is lefs need to make the cover fit clofe, only it muft be obferved, that the clofer the cover fits, the lefs the operator will be in-commoded by the fteam. The height of the barometer at which the boiling point fhould be adjufted, when this method is ufed, is 29½ inches, or three-tenths of an inch lefs than when the former method is ufed.

It will be convenient to have two or three pots of dif-ferent depths; for if a fhort thermometer is to be ad-jufted in the fame pot which is ufed for a long one, it will require a great depth of water, which, befides taking up more time before it boils, makes the obfervation rather lefs accurate, as the heat feems to be lefs regular when the depth of water in the pot is very great, than when it is lefs.

Perhaps fome perfons, for the fake of heating the water more expeditioufly, may be inclined to ufe an ap-paratus of fuch kind that the fire fhall be applied to a confiderable part of the fides of the pot as well as to the bottom; we would, however, caution them againft any thing of that kind, as the obfervations are confiderably lefs regular than when little more than the bottom of the pot is heated. If the pot is heated over a chafing-difh or common fire, we apprehend that there can feldom be any danger of too much of the fides being heated;

5 O 2

but

but if the operator fhould be apprehenfive that there is, it is eafily prevented by faftening an iron ring an inch or two broad round the pot near the bottom. This precaution is equally neceffary when the thermometer is adjufted in fteam, efpecially when there is not much water in the pot.

The greateft inconvenience of this method of adjuft-ing the boiling point is the trouble of keeping a proper depth of water in the pot, as to do this it is neceffary firft to find the height of the boiling point coarfely by try-ing it in an open veffel, and then to put fuch a quantity of water into the pot that it fhall rife from one to three or four inches above the ball, when the thermometer is placed at fuch a depth within the pot that the boiling point fhall rife very little above the cover. The operator muft be very careful that the quantity of water in the pot be not fo fmall as not intirely to cover the ball.

A third way of adjufting the boiling point is, to wrap feveral folds of linen rags or flannel round the tube of the thermometer, and to try it in an open veffel, taking care to pour boiling water on the rags, in order to keep the quickfilver in the tube as nearly of the heat of boiling water as poffible. The beft way is to pour boiling water on the rags three or four times, waiting a few feconds be-tween each time, and to wait fome feconds after the laft

time

time of pouring on water before the boiling point is marked, in order that the water may recover its full ftrength of boiling, which is in good meafure checked by pouring on the boiling water.

In this method the boiling point fhould be adjufted when the barometer is at 29.8 inches, that is, the fame as when the firft method is ufed; the water fhould boil faft, and the thermometer fhould be held upright, with its ball two or three inches under water, and in that part of the veffel where the current of water afcends[m].

Whichever of thefe methods of adjufting the boiling point is ufed, it is not neceffary to wait till the barometer is at the proper height, provided the operator will take care to correct the obferved height according to the following table.

(m) In a veffel of boiling water one may almoft always perceive the current of water to afcend on one fide of the veffel, and to defcend on the other.

Height of the barometer when the boiling point is adjusted according to,		Correction in 1000ths of the interval between 32° and 212°.	Height of the barometer when the boiling point is adjusted according to,		Correction in 1000ths of the interval between 32° and 212°.
1st or 3d method.	2d method.		1st or 3d method.	2d method.	
	30.64	10	29.69	29.39	1
	53	9	58	28	2
30.71	41	8	47	17	3
59	29	7	36	06	4
48	18	6	25	28.95	5
37	07	5 (lower.)	14	84	6 (higher.)
25	29.95	4	03	73	7
14	84	3	28.92	62	8
03	73	2	81	51	9
29.91	61	1	70	—	10
80	50	0	59	—	11

To make use of this table, feek the height which the
barometer is found to ftand at in the left-hand column,
if the boiling point is adjufted either in the firft or third
method, and in the fecond column if it is adjufted in the
fecond method; the correfponding number in the third
column fhews how much the point of 212° muft be placed
above or below the obferved point, expreffed in thou-
fandth parts of the interval between the boiling and
freezing point: for example, fuppofe the boiling point

is

is adjusted in steam when the barometer is at 29 inches, and that the interval between the boiling and freezing points is 11 inches; the nearest number to 29 in the left-hand column is 29.03, and the corresponding number in the table is 7 higher, and therefore the mark of 212° must be placed higher than the observed point by $\frac{7}{1000}$ of the interval between boiling and freezing, that is, by $\frac{11 \times 7}{1000}$, or .077 of an inch.

This method of correcting the boiling point is not strictly just, unless the tube is of an equal bore in all its parts; but the tube is very seldom so much unequal as to cause any sensible error, where the whole correction is so small. The trouble of making the correction will be abridged by making a diagonal scale such as is represented in fig. 5.

It is not very material what kind of water is used for adjusting the boiling point, so that it is not at all salt; only, if any kind of hard water is used, it is better that it should be kept boiling for at least ten minutes before it is used. But we would advise all those desirous of adjusting thermometers in the most accurate manner for nice experiments, to employ rain or distilled water, and to perform the operation in the first mentioned manner, that is, in steam.

On the freezing point.

In adjusting the freezing as well as the boiling point, the quickfilver in the tube ought to be kept of the fame heat as that in the ball. In the generality of thermometers, indeed, the diftance of the freezing point from the ball is fo fmall, that the greateft error which can arife from neglecting this precaution is not very confiderable, unlefs the weather is warmer than ufual; but as the freezing point is frequently placed at a confiderable diftance from the ball, the operator fhould always be careful either to pile the pounded ice to fuch a height above the ball, that the error, which can arife from the quickfilver in the remaining part of the tube not being heated equally with that in the ball, fhall be very fmall; or he muft correct the obferved point, upon that account, according to the following table:

Heat of the air.	Correction.
42°	.00087
52	.00174
62	.00261
72	.00348
82	.00435

The

The first column of this table is the heat of the air, and the second is the correction expressed in 1000th parts of the distance between the freezing point and the surface of the ice: for example, if the freezing point stands seven inches above the surface of the ice, and the heat of the room is 62, the point of 32° should be placed 7 × .00261, or .018 of an inch lower than the observed point. This correction also would be made more easy by the help of a diagonal scale, similar to that proposed for the boiling point.

On the precautions necessary to be observed in making observations with thermometers.

In trying the heat of liquors care should be taken that the quickfilver in the tube of the thermometer be heated to the same degree as that in the ball; or, if this cannot be done conveniently, the observed heat should be corrected on that account: but for this we refer to the former part, p. 835.

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7

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I N D E X

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OF THE

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A.

fpheroid or cylinder, having a rotatory motion about any momentary axis, is computed, p. 289—295.

Cat. A coated phial may be charged with electrical sparks from the back of a cat, strong enough to pierce a hole through a card with its discharge, p. 396. A species of pole-cat (Viverra Putorius), found in Africa, p. 40.

Cavallo, Mr. Tiberius, his account of some new electrical experiments, p. 48. See *Electrical experiments*. His experiments and observations on Mr. Volta's electrical machine, p. 116—118. His new electrical experiments and observations; with an improvement of Mr. Canton's electrometer, p. 388. See *Electrical experiments*.

Cavendish, Mr. accounts for a disagreement between the pear-gage and the other gages in an air-pump, p. 622—624.

Chocolate, on its electricity; and the restoration of that property to it, when lost, by melting it together with a small quantity of olive oil, p. 91—97.

Cisterns. Fixing the scale of barometers with cisterns, recommended, p. 405, 406.

Clausthal, in the Hartz, depth of some mines there determined by barometrical observations, p. 408—412. 431—435. Its height above Gottingen determined by the barometer, p. 429. 444—449.

Clock. A pendulum-rod of solid glass dilates much less than a steel one, p. 698, 699.

Coated phial may be charged with electrical sparks strong enough to pierce a hole through a card with its discharge, from a piece of new flannel, a hare's skin, or a piece of paper, p. 396.

Colours. An account of persons who could not distinguish colours, p. 260.—Harris, who had otherwise a good sight, discovered a want of the idea of colours at four years of age, p. 261. Why he might not observe this defect earlier, ibid. Could not discern the difference of colour in cherries, p. 262. He could only guess the name of any colour, except distinguishing white from black, or black from any light or bright colour, p. 262, 263. He could distinguish a striped ribbon from a plain one, but could not tell the colours, p. 262. Was very desirous of understanding the nature of light and colours, p. 263. He had two brothers, who likewise had no idea of colours, ibid. Account of one of them, p. 263—265.

Colours. Electrical Experiments on water-colours. p. 392—394. On oil-colours, p. 394, 395.

Committee. The report of the committee appointed by the Royal Society to consider of the best method of adjusting the fixed points of thermometers; and of the precautions necessary to be used in making experiments with those instruments, p. 816. See *Thermometers*.

Cooch Behar. Europeans and Boutaners first met at its attack, p. 467. Their great surprize at each other, p. 467, 468. Boutaners, conquered by the Europeans, sue for peace, p. 468.

Copernicus, an account of a portrait of him, presented to the Royal Society by Dr. Wolf, p. 33. Copied from an original in the possession of Mr. Huffarzewski, p. 33, 34. A portrait of him in the great church at Thorn, p. 34. Compared with Mr. Huffarzewski's,

Delai

of

of the motion of the earth's axis, p. 267—288. How the joint centrifugal force of the particles of a fpheroid or cylinder, having a rotatory motion about any momentary axis, is computed, p. 289—295. The ratio of the earth's diameters, p. 767. Beſt method of determining the figure of the earth, ibid.

Eaſt Indies. The atmofphere amazingly clear there, p. 604.

Ebn Younes. Tranſlation of a paſſage in Ebn Younes; with ſome remarks thereon, p. 231. Numbers in the manufcript fuppofed to be different from what they were in the original tables, ibid. A miſtake in the Arabic letters and figures eafily made by a perfon ignorant of his fubject, p. 232. An inſtance of this fort, ibid. Differences in the manuſcript obſervations, and the calculations by modern tables accounted for, p. 232—240. A tranſlation of the Arabic paſſage entire, p. 240—243.

Eclipſes of Jupiter's firſt ſatellites, obſerved during the laſt ten years, relatively to the difference of longitude at Paris and Greenwich refulting therefrom; with a comparative table of the correfponding obſervations of the firſt ſatellite, made in the principal obſervatories, p. 162—186.

Edinburgh, computations of barometrical obſervations made on heights near there, p. 777.

Electrical Experiments, an account of ſome new ones, p. 48. Defcription and ufe of the atmofpherical electrometer, p. 48, 49. Part of a journal of obſervations on the electricity of the atmofphere, p. 50. A quantity of electricity is at all times in the atmofphere, ibid. The electricity of the atmofphere or fogs is always of the fame kind, ibid. The ſtrongeſt electricity is in thick fogs; the weakeſt in cloudy weather, ibid. Defcription of the electrometer for the rain, p. 51, 52. Experiments made with a glafs tube hermetically fealed, and having ſome quickfilver inclofed in its cavity, p. 53—55. New electrical experiments and obſervations, with an improvement of Mr. Canton's electrometer, p. 388. The glafs plate ufed in the following experiments coated with fealing-wax, ibid. Experiments on Mr. Volta's commonly called a machine for exhibiting perpetual electricity, p. 389—392. riments on watercolours, p. 392—394. On oil colours, p. 394, 395. ſious experiments, p. 396, 397. On a piece of new white flannel, tied r.. e globe of an electrical machine, p. 397, 398. On a phial, the infide coating of which was ſtuck with varnifh, p. 398, 399. Defcription of a pocket electrometer, p. 399, 400.

Electrical machine, experiments and obſervations on Mr. Volta's, p. 116—118. Experiments on his machine for exhibiting perpetual electricity, p. 389—392.

Electrical cylinder (Mr. Hemming's), experiments with it on the balls of Canton's electrometer, ſome days after ufing it, p. 109—113.

Electricity. Experiments and obſervations in Electricity, p. 85. Remarks on the effects of lamp-black and tar, or lamp-black and oil, as protectors of bodies from the ſtroke of lightning, p. 85—89. Similar effects produced by experiments in the artifi-

cial electricity, p. 89—91. Corollaries deduced from the above experiments and obfervations, p. 91—94. On the electricity of chocolate, 94—96. The reftoration of that property to it, when loft, by melting it together with a fmall quantity of olive-oil, p. 96, 97. Remarks on electricity, 97, 98. Obfervations on fome new and fingular phenomena in excited and charged glaffes; with experiments made in confequence of thefe phenomena, farther illuftrating the Franklinian theory of the Leyden bottle, p. 98—116. A defcription of the apparatus conftructed for that purpofe by Mr. Henly, p. 99, 100. Experiments and obfervations on Mr. Volta's machine, with remarks, p. 116—118. A fimple electrical apparatus, by which almoft every article may be examined with the utmoft facility, p. 118. Obfervations and precautions in the ufe of it, p. 119, 120. The electricity of various fubftances difcovered with it, p. 122. Metals, ibid. Animal fubftances, p. 123. Vegetables, p. 124, 125. Corallines, p. 125. Salts, ibid. Foffil and mineral fubftances, p. 125, 126. Artificial fubftances, p. 126. Remark on the rubbers ufed, p. 127—129. Thoughts on the action, influence, and effects of electricity, p. 129—144. Obfervations on the electricity of the atmofphere, p. 50. A quantity of electricity in the atmofphere at all times, ibid. The electricity of the atmofphere and fogs of the fame kind, ibid. Strongeft electricity is in thick fogs; the weakeft in cloudy weather, ibid. Electricity may be obtained by rubbing fmooth glafs with a rabbit's fkin, p. 397.

Electrometer, an atmofpherical one, its defcription and ufe, p. 48, 49. Defcription of one for the rain, p. 51, 52. Defcription of a pocket one, p. 399, 400. Mr. Hemming's experiments with his electrical cylinder, fome days after ufing it, on the balls of Canton's electrometer, p. 109—113.

Emelman, Mr. his journey into Africa from the Cape of Good Hope, p. 38. See *Africa*.

England, the evaporation of fome parts of it, p. 253. Not correfpondent with that at Liverpool, ibid.

Evaporation. Obfervations on the annual evaporation at Liverpool; and on evaporation confidered as a teft of the moifture or drynefs of the atmofphere, p. 244. Quantity of rain falling not fo good a teft of the ftate of the atmofphere as evaporation, p. 244, 245. Method of determining the annual evaporation in Liverpool, p. 245, 246. Explanation of the tables of evaporation, p. 246. A comparative view of the evaporation, rain, winds, and temperature of the air there, during the years 1772, 1773, 1774, and 1775, p. 247—250. Depth of rain not a true index of the moifture or drynefs of the atmofphere, p. 250—252. Why much rain gives no more moifture to the atmofphere than a little, p. 251. Annual evaporation at Liverpool, p. 252. Evaporation at London, p. 252. Annual evaporation at London exceeds that at Liverpool, ibid. Exhalation from water at Delft in Holland, ibid. Evaporation of fome parts of England, p. 253. Not correfpondent with that at Liverpool, ibid. Annual evaporation from water fix times as much as from the earth, ibid. More falls in

rain

F.

Fasting

Gold,

K.

L.

M.

N.

O.

Q.

Queen-bees produced from the common eggs, p. 28—30. The working-bees have the power of making a queen of any worm in the hive, p. 30, 31. The received opinion that the queen-bees are produced from a particular kind of eggs, erroneous, p. 31.

Quickſilver. An experiment to determine hydroſtatically the ſpecific gravities of air and quickſilver, with a given temperature and preſſure, and remarks thereon, p. 557—570. Expanſion of quickſilver in the tubes of Mr. De Luc's barometer, reſulting from the Saleve obſervations, p. 656—658. Experiments on the expanſion of quickſilver, p. 659—681. Rate of expanſion of a column of quickſilver in the tube of a barometer, p. 682. Conſtruction and application of a table of equation, for the expanſion of quickſilver in the tubes of barometers, p. 683—686. The equation to be applied to the obſerved height of quickſilver in the barometer, from 15 to 31 inches; and for differences of temperature extending to 102° of Fahrenheit; whereby the column is reduced to the height it would have ſtood at in the temperature of 32 degrees, p. 687. The expanſion of common air does not keep pace with the dilatations of quickſilver, p. 695.

R.

Rabbit's ſkin, a coated phial may be charged with electrical ſparks from one, ſufficiently to pierce a hole through a card with its diſcharge, p. 396. Smooth glaſs rubbed with a rabbit's ſkin acquires electricity, p. 397.

Rain. Deſcription of an electrometer for the rain, p. 51, 52. No rain ever in the kingdom of Peru, p. 257. The quantity of rain falling not ſo good a teſt of the ſtate of the atmoſphere as evaporation, p. 244, 245. A comparative view of the rain at Liverpool during the years 1772, 1773, 1774, and 1775, p. 247—250. Depth of the rain not a true index of the moiſture or dryneſs of the atmoſphere, p. 250—252. Why much rain gives no more moiſture to the atmoſphere than a little, p. 251. More rain falls than is raiſed in vapour, ſuppoſing the whole to be a ſurface of water, p. 254. Six times more deſcends in rain than what is exhaled from the earth, ibid. How the rain is collected and reſtored to the ſources from whence it came, p. 254, 255. Annual fall of rain between Lancaſhire and Yorkſhire, p. 255. Exceeds that of Liverpool, ibid. The quantity of rain which fell at Lyndon in Rutland during the year 1776, p. 350. What quantity fell in Briſtol in the ſame year, p. 354. State of the rain for each month in London, throughout the ſame year, p. 358—381. Quantity which fell in each month, and in the whole year, p. 382.

Rain-gage, wrong method of fixing it, p. 255.

Ramelſberg near Goſlar, in the Hartz, method of working the lead-mines there, p. 414. Depth of mines there determined by barometrical obſervations, p. 417—419. 436—440.

Ramſden, Mr. deſcription of his portable barometer, p. 658, 659.

Raumur,

a com-

T.

Result

5 T 2

of

tics, p 473—479. Some account of their buildings, p. 479. Of its capital and trade, p. 481—487. Tranflation of a letter fiom the Tayfhoo Lama to the governor of Bengal, foliciting a peace, p. 489—492.

Thorn, a portrait of Copernicus in the great church there, p. 34.

Tides. An account of the tides in the Adriatic, p. 145—161.

Time, the Bramins method of reckoning it, p. 606.

Toaldo, the Abbé Jofeph, his account of the tides in the Adriatic, p. 145—161.

Townley, Mr. his obfervations on the annual fall of rain between Lancafhire and Yorkfhire, p. 255. It exceeds that at Liverpool, ibid. An error in fixing his rain-gage, ibid.

Trees. On the ufefulnefs of wafhing and rubbing the ftems of trees, to promote their annual increafe, p. 12. Tree cleared of mofs and dirt with water and a brufh, ibid. Wafhed with a coarfe flannel, ibid. Wafhings frequent in dry times, ibid. Flourifhing ftate of the wafhed tree compared with others of the fame nature, p. 12—14. Defcription of the Jefuits' Bark-tree of Jamaica and the Caribbees, p. 504—506. Defcription and ufe of the Cabbage-bark tree of Jamaica, p. 507—512.

V.

Vapour, its effect in experiments with the air-pump, p. 625, &c. Its effect in the expanfion of quickfilver, p. 670, &c.

Vapours, the atmofphere of Peru conftantly obfcured with them during part of the year, and the whole country involved in thick fogs, p. 257, 258.

Varnifh. An electrical experiment on a phial, the infide coating of which was ftuck with varnifh, p. 398, 399.

Viverra Putorious, a fpecies of pole-cat, found in Africa, p. 40.

Volcanic-hill, an account of one near Invernefs, p. 385. Named Creck Faterick, or Peter's Rock, p. 386. Defcription of it, p. 386, 387. Evident marks of its having been a volcano, p. 386, and 387 n. No crater could be difcovered, p. 386, 387 Another hill near Dingwal, in this country, fuppofed to be volcanic, p. 387.

Volta's electrical machine, experiments and obfervations on it, with remarks, p. 116—118. Experiments on his plates, commonly called a machine for exhibiting perpetual electricity, p. 389—392.

W.

Wargentin, Mr. Peter, his letter concerning the difference of longitude of Paris and Greenwich, refulting from the eclipfes of Jupiter's firft fatellites, obferved during the laft ten years; with a comparative table of the correfponding obfervations of the firft fatellite, made in the principal obfervatories, p. 162—186.

Wafhing and rubbing the ftems of trees increafes their growth, p. 12—14.

water for four years, ibid. No evacuation by stool, and scarcely any by urine, for three years, p. 5, 6. Her pulse distinct and regular, slow and small, p. 6. Countenance clear and pretty fresh, and her features neither disfigured nor sunk, ibid. Her body felt like that of a healthy young woman, ibid. Her knees bent, and hamstrings tight as a bow-string, ibid. She sleeps much and quietly, but keeps a constant whimpering when awake, p. 7. Her mouth soft and moist, ibid. State and condition of the patient five years after the above account was taken, p. 8. A few crumbs and a little moisture her only sustenance, ibid. Jaws still fast-locked, and she never attempts to speak, p. 9. Her ham-strings tight as before, and eyes shut, ibid. Her whole person rather emaciated, ibid. Still sensible and tractable in every thing, ibid. Great improvement in her looks and health, p. 11. Takes more food, ibid. The account of this woman authenticated, p. 10, 11.——An account of a hard substance extracted from a woman's groin, p. 461—463. Caused by a peg of wood which she had swallowed sixteen years before, p. 462. *n.*

The End of the SIXTY-SEVENTH VOLUME.

The Number of PLATES *in this Volume is* TWENTY.

The second, containing two different subjects, is marked Tab. II. at the Top, and Tab. III. below. The drawing of the eleventh Plate, having been sent in too late to be numbered in the regular series, is marked Tab. Ẍ.

The letter-press tables should be bound in upon guards by the middle, to avoid any folding out; and in those which must be looked at side-ways, the page should lie towards the right-hand.

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58, 9. *for* commmunicate, *read* communicate,

85, 15. *for* XLVIII, *read* LIV.

128, 4. from the bottom, for " and not all" *read* " and not at all";

131, 16 and 17. *for* (as the millers term it when no Iron is concerned) *read* (as the millers term it) where no iron is concerned .

162, 6. *for* Satellites, *read* Satellite.

165, 9. *for* ineptats, *read* ineptas

258, 3. from the bottom, *for* but, *read* long

258, 2. from the botttom, *for* long, *read* but

354, 2. *for* the year 1775, *read* the year 1776.

475, 13. *for* creduiitity, *read* credulity

518, 7. from the bottom, *for* $\frac{1}{2000}$ *read* $\frac{1}{20,000}$.

519, 7. *for* 233°, 54′, 15″ *read* 233°, 53′. 15″

520, 4. *insert* ∠ c by 4th obfervation=9°, 59′, 0″—9°, 38′, 15″

521, 2: *for* mountains, *read* mountain.

522, 2. *for* correct for the fignal 59″, *read* 54″

530, 5. *for* 27,7025, *read* 25,7025

541, 4. *for* above at C. *read* above at B.

545, 11. *for* correct height in fathom 686,619, *read* 685,619

546, 8. *for* difference of Log. 654,157, *read* 654,109

547, 11. *for* (in p. 556), *read* (in p. 532)

556, 17. *for* two, *read* too

560, 1. *for* feet, *read* grains

        18. *for* 13358,5, *read* 13558,5.

562, 12. *for* barometer, *read* manometer :

568, 5. from the bottom, *for* $\overline{\overline{T-S} \times \overline{E-\imath-\alpha}} = S-x$, *read* $\frac{\overline{T-S} \times \overline{E-e-\alpha}}{E} = S-x.$

569, 19. *dele* the femicolon after quantity, and infert it after inftance

578, 5. from the bottom, *for* the attached Therm. *read* the two attached Therm.

585, 2. *read*, fee p. 574 and 567

        in the column for 25 inches, and againft 21 *for* 53,2, *read* 53,1

586, 3. *add*, fee p. 568 and 569

        In the 4th col. of the table at the top, *for* 16,10, *read* 15,10.

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587,   in the 2d col. againſt 19,90, *for* 12337,0, *read* 12377,8ˑ

20,70, *for* 11350,0, *read* 11350,8ˑ

588,   30,4ˑ, *for* 1436,6, *read* 1336,6

in the laſt col. *for* 81,0, *read* 81,8.

59ˑ,   2. *for* uppermoſt, *read* approximate

In plate XI. Iſt col. of the table of the ∠' and ſides of the △' for *End of the baſe* 1. 2.
    read *End, of the baſe*, 1. 2.

In the ſmall ſcale of feet at the ſide of the ſection of the mountains for 1500, *read*
    15000 feet.

608,   3. *for* July, *read* Auguſt

628,   4. from the bottom *for* 159, *read* 152ˑ

642,   13. *for* piſtons were, *read* piſton was

658,   10. *for* plate XVI. *read* plate XV.

662,   5. *for* plate XVII. *read* plate XVI.

683,   15. *after* inverſe, *inſert* ratio,

685,   5. a point *after* unaltered. *And for* but that in, *read* But in.

705,   Tab. IV. 9th experiment, column 132° to 152°, *for* 9,14350, *read* 9,14550ˑ

736,   At the end of the note, *for* Barometer, *read* Barometers

777,   1774. December 10, weight of quickſilver to air, *for* 11445, *read* 10445.

781,   13th Station, obſerved ſuperior barometer, *for* 25,691, *read* 25,961

785,   3d Obſervation at Belmont Caſtle, inferior equated barometer, *for* 29, 64.
    read 29,664

819,   7. *for* were the ſhort, *read* were ſhort

821,   5. *for* 15 *read* 16.